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"CREDULOUS" AND "SCEPTICAL" VIEWS OF HYPNOTIC PHENOMENA

A Review of Certain Evidence and Methodology

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A traditional opposition runs through the many viewpoints about the nature of hypnosis. It began with Mesmer and Benjamin Franklin (Binet), repeated itself with Elliotson and Braid (Bramwell 1906) and again with Charcot and Bernheim, and finds its modern form in the views of Erickson (LeCron) and White. What might be called the "credulous" view takes the hypnotic subject's testimony on faith. For example, it is suggested to a subject in trance that a blank card is a photograph of a person's face; if the subject says that he can see the face and goes on to describe its detail, the "credulous" view is that in some sense the subject does "see". The perceptual process is assumed to be akin to that which is produced by an actual set of stimulus conditions. Erickson (1937) is quite explicit in his view that hypnotically produced "unconsciousness", "deafness" (1938, 1944), "colour blindness", and "after-images" (1938) are like the naturally occurring organic conditions. In opposition there are those who doubt the subject's testimony and contend that he actually perceives the situation as it is while acting as if it were as suggested by the hypnotist. This might be called the "sceptical" point of view; and White's position is of this sort. The "credulous" and "sceptical" viewpoints diverge on certain questions of fact, particularly on that of the subject's perceptions, and it is of interest to take stock of the large body of pertinent evidence which has been accruing. This must be considered in light of criteria of admissible evidence. This paper examines the minimum requirements of experimental control in hypnotic experimentation, and reviews evidence from studies of hypnotically produced aesthesias; auditory, visual and temporal hallucinations; regression; and anti-social behaviour.

Methodological rigor has not been characteristic of studies of hypnosis. The works of Young (1926) and Hull have historical significance and have influenced the development of the subject because, contrary to earlier and some subsequent work, they paid due attention to experimental requirements. An analysis of successful and unsuccessful investigation through the history of hypnotism reveals a minimum set of re-

quirements for any given experiment to be conclusive. Unfortunately as many recent studies fail in this respect as did early ones. Thus it will aid in the evaluation of evidence to set down the controls necessary in an experiment on hypnosis.

Young (1926) was perhaps the first to show that hypnotic behaviour must be compared with "normal waking behaviour" if causation is to be attributed to hypnosis. Without this comparison one cannot show that hypnosis has any effect, and it is a mistake to attribute automatically any given behaviour that occurs in trance to hypnosis; it may occur as readily without. For example, Young's subjects in trance were remarkably acute in discriminating postage stamps but they performed as well in the waking state; thus hypnosis was not the cause of the performance, and the latter was remarkable only because of its novelty. It may seem obvious to use waking controls, but many studies of hypnosis fail to do so.

The form of the waking control is important. If the one subject is used in the trance and waking states there is the general problem of transfer effects and a further problem peculiar to hypnosis. Pattie (1933) shows how the subject may bring the trance and waking performances into consistence with the relationship expected by the hypnotist or demanded by by the subject's own situation. (See also Orne, 1959). For example, if the subject in the waking state is asked to simulate his previous trance performance, to do so successfully would be tantamount to an admission that his trance behaviour was simulation; accordingly he may perform in a way different from the norm for "waking", and in a way which he anticipates the hypnotist to expect. Pattie points out that there is difficulty also with the reversed order of experimentation, as the subject may gain experience in a control session which can facilitate successful simulation in a subsequent trance. Accordingly, independent subjects are needed for waking and trance groups unless the situation is such that the subject is not compromised, is not aware of the hypnotist's expectations, or such knowledge cannot affect the waking performance. Few studies use independent waking controls.

Where independent controls are used, it is necessary that the trance and waking groups be comparable. This is not always ensured, especially when the waking controls are made up of the most readily available subjects. The trance group will be highly selected for hypnotizability, whereas a haphazardly selected waking group will be at least more heterogeneous. Not uncommonly it will be made up of recalcitrant or non-hypnotizable subjects met with in the selection of trance subjects. This leads to a confounding of subject differences with the trance-waking comparison. What is needed is a homogeneous pool of hypnotizable sub-

jects from which two independent random samples may be drawn, one to be the trance group, one the waking control. Little attention has been paid to sampling procedures in hypnotic experimentation.

The condition "hypnosis" itself can be analysed at least into (i) the trance state or result of induction procedures; and (ii) trance behaviours in response to suggestions given after the trance is achieved. Induction procedures may operate independently of the experimental trance suggestions to affect the dependent variable. For example, slower performance on a timed task may be due to trance suggestions or quite independently it may be due to the use of sleep suggestions in the induction of trance. To control for this possibility one would need two comparable but independent trance groups, one to perform the experimental task in trance with experimental suggestions, the other to perform the experimental task in trance but without the experimental suggestions. Few studies utilize this distinction in their design.

The hypnotist may be a source of systematic error. One example of this pertains to the extent to which the hypnotist's investment in a given experimental outcome is implicitly communicated to the subjects such that the way the results come out is unwittingly arranged (Orne, 1959 for a discussion of the problem of "demand characteristics"). Replication of experiments with independent hypnotists is desirable. One other variable of some relevance is the subject, specifically his hypnotizability. Failure to demonstrate a phenomenon with a non-hypnotizable subject does not show that it will not occur with a hypnotizable subject, and so both types of subjects should be used. Related to this is the question of the time and care taken with induction procedures (Erickson in LeCron 1952). In addition to these various general considerations, in any given experiment there will be needed certain specific controls. These various criteria will be used in the evaluation of evidence which follows.

Studies of Aesthesia

Almost all studies in this area have concerned hypnotic anaesthesia. Anaesthesia as a spontaneous (rather than a suggested) feature of hypnosis was recognized quite early in the 19th century; and Esdaile, Elliotson and Braid all made use of it for surgical purposes (Bramwell, 1906, Ch. ix). Bernheim (p. 116) cites isolated cases of major amputations and says ..."In spite of these fortunate trials, surgeons soon showed that hypnotism only rarely succeeds as an anaesthetic, that absolute insensibility is the exception among hypnotizable subjects, and that hypnotizing itself fails in persons disturbed by the expectation of an operation." The Paris Commission of 1837 (Binet, p. 43) had noted that ..."the subject of the experiment was as insensible to pinpricks

before the supposed sleep as when it occurred." This raises the possibility in the absence of waking controls on surgical work that "hypnotic anaesthesia" is irrelevant to the subject's insensitivity. That particular individuals undergo surgery, have teeth pulled or drilled, and give birth apparently without pain while hypnotized is not in itself evidence that anaesthesia has been produced by the hypnosis. Such subjects may be generally insensitive to pain through heredity, the effects of progressive disease, etc. What are needed to clarify the significance of these sorts of observations are norms for waking sensitivity to various kinds of painful stimuli as applied to different areas of the body.

Experimental evidence on hypnotic anaesthesia can more readily be brought to bear on the "credulous" versus "sceptical" issue. Levine investigated hypnotic anaesthesia using S's report of pain and the GSR as a physiological indicator. He noted that hysterical patients give the GSR to painful stimulation of "anaesthetic" areas. Using a veni-puncture needle through the skin of the forearm, he found that the hypnotically anaesthetic subject reported no pain but gave the GSR just as did the hysteric. Dynes measured the respiration, heart rate and GSR of six subjects in response to pinch and pinprick in the waking state and with hypnotic anaesthesia. He found that while respiration was slower in hypnosis, there were no differences between the states for the other measures.

Sears using seven subjects measured facial flinch, reflex leg withdrawal, respiration, pulse and GSR in response to a barb forced against the calf of the leg. Subjects were tested in the waking state and in hypnotic anaesthesia, and subsequently were requested to voluntarily inhibit pain reactions in the waking state. Hynotic anaesthesia produced marked reduction in facial flinch and verbal report of pain—both subject to voluntary inhibition. Some differences were noted for the remaining measures, but when individual reactions were examined there was found no uniformity of response to favour hypnotic anaesthesia and the overall mean differences are attributable to an occasional subject in each case. Larger samples would be required to establish a reliable trend. The GSR difference is negligible. While the subsequent "voluntary" inhibition of pain reaction in the waking state was not as successful as the hypnotic anaesthesia, these results are suspect in terms of Pattie's (1933) criticism. Sears himself points out that non-inhibition of facial flinch suggests that the subjects did not really try to inhibit the pain reaction in this experimental condition. With an independent control group the results could well have been otherwise (Orne 1959).

Brown and Vogel followed up a suggestion of Hull for a comparative study of chemical and hypnotic anaesthesia. With three subjects they

measured blood pressure, pulse, respiration and GSR in reaction to both sharp and continuing pain. They found that hypnotic anaesthesia is not as successful as chemical anaesthesia in reducing pain reaction; that, while hypnotic anaesthesia served to reduce the various reactions, waking simulation was just as successful (see also Orne 1959). Most recently West et al. measured the GSR of seven subjects in response to four degrees of radiant heat. They compared the subject's waking reactions to pain with reaction under hypnotic anaesthesia, and they found for individual subjects reduction of from 26 to 67 percent in GSR. The data as reported do not show the effects of the different degrees of pain, and furthermore it can not be determined whether the reductions were other than adaptation effects.

Pattie (1937) studied anaesthesia to touch. The task was for a blindfolded subject, with his hands in position for the Japanese illusion, to count the number of times the left hand was touched by E, the right hand being "hypnotically anaesthetic". A group of 15 "normal" subjects established the basal count for comparison with the results of an independent group of 5 "trance" subjects. If truly anaesthetic, the trance subjects would perform better than the norm for waking performance. While the results showed differences in the right direction for the first two trials, the overall trend for 10 trials is almost identical for the two groups. As the actual number of touches made by the experimenter is not reported proportional error cannot be gauged and the relative magnitude of the initial difference is unknown.

In summary: All experiments reveal at least some reaction to stimuli for which hypnotic anaesthesia is said to hold. Most experiments reveal some reduction in reaction to stimuli with hypnotic anaesthesia when the latter is compared with the "normal" reaction. Reduction is greatest with reactions subject to voluntary control, and least or non-existent with autonomic functions such as the GSR. However, voluntary inhibition seems to be as effective as hypnotic anaesthesia in achieving such reductions. Such disagreement as exists amongst the studies may be due to method. The need for larger samples and for independent waking controls is apparent. Standardization of measurement procedures could eliminate other differences. In general these data do not support the "credulous" view that by hypnosis there can be produced a genuine absence of painful sensory content insofar as this is indicated by behavioural reactions. The puzzle remains that hypnotically anaesthetic subjects report in many cases that they feel no pain. Future research might well be directed to resolving this discrepancy between the evidence of bodily reaction and reported subjective experience.

Studies of Hallucination

Auditory

In this area studies almost exclusively concern hypnotic deafness, the negative auditory hallucination. Exceptions dealing with positive auditory hallucinations are (Lundholm, 1928, 42, Sterling).

From the observation that a hypnotic subject appeared deaf to all but the hypnotist's voice, it was but a short step to the suggestion of deafness. Bernheim (p. 30) wrote... "I induce deafness. The subject says that he hears nothing. He answers nothing and does not react to the most deafening noises." He goes on to point out a puzzling feature of the subject's behaviour (p. 50). "I say to a somnambulist who has been hypnotized: 'When you wake you will not see me; you will not hear me; you will be deaf and blind.' I wake him; I speak to him and whisper in his ear. He does not show any sign of understanding: his face remains inert. If I then say to him decidedly, perhaps once or several times: 'You hear again'; his face shows great astonishment; he hears and answers me. It is in vain that I say 'You must have heard me all the time, since your pretended deafness vanished when I assured you that you again heard me'."

In the modern period, Lundholm (1928) set a pattern for experimental work on this problem. He sought to condition finger withdrawal to an auditory stimulus for which deafness had been suggested in trance. He found an absence of response under these conditions; and also, when deafness was induced for a CS to which the subject had already been conditioned, the conditioned response disappeared. Lundholm noted that the subjects' responses in these cases were unlike ordinary conditioned responses and were more like voluntary inhibitions.

With non-voluntary responses, hypnotic deafness is not sufficient to abolish the usual reaction to noise. Dynes found no significant difference in GSR to a pistol shot from the waking to the hypnotically deaf state. Brown and Vogel (p. 420) make the incidental comment: "Suggested deafness does not abolish physiological reactions to auditory stimuli." Lundholm and Lowenbach investigated the alpha rhythm of the EEG and found it to disappear with noise whether the subject was in the waking state or hypnotically deaf; while it continued with quiet whether the subject was awake or positively hallucinating a noise. Sterling and Miller found no evidence that hypnotic suggestion increases auditory acuity.

Pattie (1950) investigated the possibility of inducing unilateral hypnotic deafness. Of 12 trance subjects, 4 reported that they could not hear a tone presented to the "deaf" ear. To check, two tones one to each ear

were presented simultaneously but at slightly different frequencies. Under these conditions if the subject hears with both ears he will perceive beats; if he hears with only one ear he will perceive a single tone. All four "deaf" subjects reported hearing beats, 2 of them on all occasions, and 1 erratically with single tones reported for the remaining occasions, and 1 initially but thereafter switching to consistent report of a single tone.

Erickson (1938, 1944) has claimed positive results. In the first study he began with 100 college students of whom 70 were found to be capable of... "the more stuporous trance and the profound somnambulistic state as contrasted with ordinary deep hypnotic sleep." Of these he used 30 who were "trained" until... "it was possible to secure the stuperous trance and the somnambulistic state within 10 minutes. On a given day, 20–40 minutes were used to induce deafness, whereupon a number of clinical tests of deafness were applied. He contends that 6 subjects were totally deaf and 4 partially deaf on the evidence of these tests. Some of the subjects were asked to simulate their deafness in the waking state but they did not duplicate their trance performances. He concluded... "That the clinical tests employed, despite their variety and directness or indirectness, were entirely adequate to detect complete absence of response is open to question; but the extensiveness of the auditory changes cannot be doubted. The conclusion is warranted that there was produced a condition not distinguishable from neurological deafness by any of the ordinary competent tests employed."

In a follow up study, Erickson (1938) used four of the subjects who had exhibited "complete deafness" on the clinical tests and conditioned finger withdrawal to shock (UCS) to a buzzer (CS). Having tested for the presence of the CR, he then induced deafness and gave a further test for the CR; in the latter case it was found to be absent. With "deafness" removed the CR was found to reappear. In a not strictly comparable situation when awake the subjects were not able to inhibit the CR.

Following observations (Malmo, 1952) on an hysterically deaf subject, Malmo et al. (1954) made an investigation along Erickson's lines with "hypnotic deafness". The hysterical subject had shown some evidence of startle to loud noises, and of increased muscular tension in a conditioned finger withdrawal set up. That is, the subject was hearing sounds but he was not responding to them in the normal way—a distinction discussed by Erickson (1944). Malmo et al. then used two hypnotic subjects in whom deafness was induced via Erickson's procedures. These subjects were compared with an organically deaf control subject. Changes in the electrical potential of the muscles were recorded in re-

sponse to loud tones. The "hypnotically deaf" subjects initially gave strong reactions to the tones which reactions subsequently decreased. The decrease is a normal habituation effect. Simulated deafness in the waking state was not as successful as hypnotic deafness in reducing reaction. Eyeblink in response to auditory stimuli was present with all subjects in all test situations.

A third study of this sort by Kline et al. (1954) provides similar results with a different method. One subject, while awake and while hypnotically deaf, read aloud under conditions of delayed speech feedback. The effect of trance per se was checked by having the subject read without delayed feedback in the trance and in the waking states. They found evidence of resistance to feedback distraction, although the hypnotically deaf performance was not comparable to performance in the absence of distraction.

In summary: While there is more diversity of findings with "hypnotic deafness" than with "hypnotic anaesthesia" the general impression is the same. The presence of autonomic reactions similar to those found in the waking state indicates that the subject does perceive the stimuli for which hypnotic deafness is alleged. Where inhibition is noted, the inhibited response is also subject to voluntary control in the waking state. Differences is degree may be explicable in terms of motivation (Orne, 1959) and failure to use independent waking controls. While Erickson's (LeCron) contentions about depth of trance may apply to a number of studies, this is not true of (Kline 1954, Malmo 1954); and Erickson's own studies are open to criticism on several grounds; (i) no quantitative data are presented and conclusions are drawn from a single illustrative conditioning record; (ii) his conditioning set up involved simultaneous presentation of CS and UCS, and as with Lundholm's (1928) earlier study a "voluntary" CR, both of which features make it doubtful that conditioning proper occurred; (iii) the bias arising from the selection and training of his subjects is an unknown. Thus while Erickson's findings are at variance with the general trend, they are not conclusive and a repetition of his study with appropriate controls would be needed before the "credulous" viewpoint could be sustained.

Visual

Four areas of investigation may be distinguished—positive and negative hallucinations involving vision generally and colour vision. The general material will be considered first.

While Braid (Bramwell, 1906) was aware of suggested visual hallucinations, the first experimental work was conducted by Binet (1887) working on Fere's assumption that ... "the imaginary object which fig-

ures in an hallucination is perceived in the same way as a real object". He investigated this notion with optical methods: pressure on S's eyeballs to give double image; interposition of opaque objects; prism; opera glasses; mirror. In general the subject would hallucinate an object on a plain surface and would report the behaviour of the object under optical distortion. Binet often found that the subject's response was in accord with the behaviour of real objects, and he concluded that hallucinations are associated with (real) points of reference which are subject to optical laws.

Bernheim (p. 99ff.) also investigated this problem and showed that hallucinations are independent of particular points of reference, though the latter may be used as cues to an "appropriate" hypnotic response. He took the precaution of using naive subjects and worked in silence to minimize their knowledge of his expectations. He had subjects hallucinate in free space—the sky or in the dark—and in these cases Fere's formula breaks down. Incidental to Binet's enquiry was the observation that subjects can make use of minute cues, e.g. to recognize one of a set of blank cards, and it was thought that hypnosis increased visual acuity. Young (1926) using the appropriate waking controls found no advantage of hypnotic over waking performance at discriminating postage stamps. Sterling and Miller and Eysenck confirmed this finding; while Weitzenhoffer (1951) varying task difficulty found some advantage for the hypnotic state. The latter did not use independent waking controls.

Concerning negative visual hallucinations, Bernheim (p. 47) described a test devised by Stoeber to detect "physical blindness". It uses red and green filters in the perception of mixed red and green letters. Bernheim found that hysterical and hypnotic blindness were distinct from neurological blindness, naive subjects reading the letters indiscriminately. In 1933, Pattie tested five subjects for hypnotically suggested uni-ocular blindness with: filters, stereoscope, Flees Box, opthalmological tests, plot of the blindspot. Two subjects rejected the suggestion of blindness; and of the three who accepted it, one reported blurring of vision and the other two claimed inability to see. Four failed the tests. The fifth was investigated further and finally failed on a complicated filter test. Investigation led to the subject revealing methods whereby (although seeing) she had managed to perform on the tests as if blind.

Dorcus found no modification of the pupilliary reaction to light with suggested blindness. On the other hand, Loomis, et. al. reported EEG activity in suggested blindness akin to actual blindness. The subject's eyes were taped open, and it was suggested in trance that the subject would be blind and sighted at alternate 15 second intervals. Alpha trains occurred when S was blind and disappeared when he was sighted. There

was no waking control on S's performance. Lemere points out that EEG activity is not alone affected by visual stimulation ... "a uniform visual field of light or dark, or even a field containing blurred indefinite outlines, usually does not interrupt the alpha rhythm" ... "a few people do not have the alpha rhythm with the eyes open or shut". What is required for the disappearance of alpha rhythm is active novel stimulation such as where S examines and describes some object. In light of these points, Loomis' study would need repetition with more subjects. Lundholm and Lowenbach with three subjects studied hypnotically induced positive and negative visual hallucinations. All subjects showed marked alpha activity in a control session; and there were no individual differences when visual stimuli were presented—the alpha rhythm disappeared. In the "hallucinated" states the EEG responded to the real state of affairs rather than to the state suggested: alpha disappeared when new stimuli were introduced; and alpha continued in the absence of novel stimuli even though their presence was positively hallucinated.

In summary: With one exception these findings are in accord with the impression gained in foregoing sections. The data show the subject to be acting as if he were able to see a non-existent object, or blind to a real object; and there is little to support the "credulous" view that the subject has the appropriate sensory content.

Work on colour vision anomalies also has a long history. Following studies by Parinaud (1882) on chromatic contrast with hallucinated colours, Richet, Binet and Fere (1884) investigated the possibility of after-images to hallucinated colours and (Binet, 1887, p. 252–3) ... "ascertained that hallucinatory vision is subject to the same conditions (as normal vision); every hallucination of some persistence is succeeded on its disappearance by an after image, just as in the case of ordinary sensations which affect the retina". The method used could be responsible for their results. They wrote (p. 254) ... "to ensure success, care must be taken to define the nature of the suggested colour. For instance, if only the colour *red* is suggested, the subject may either see the shade of red of which green is the complementary colour, or the orange-red of which blue is complementary. These contradictory results are impossible when the colour which the subject is intended to see is made clear by a comparison." While it is necessary to specify a particular colour, this method is unsuitable in that it gives the subject an opportunity to discover the exact after-image required by looking at the real comparison colour. Bernheim used naive subjects and took care not to reveal what was expected and in this case the results were negative.

Binet and Fere also claimed to have observed complementary after-

images to vivid waking images. If this were so, hypnotic suggestion would facilitate rather than initiate their occurrence in trance. To check this claim, Downey studied the imagery of a group of trained introspective observers. She found considerable individual differences in after-images to *real* stimuli. She tried "a great many subjects" without finding after-images to mental images. One subject, in her opinion, showed some evidence of a positive kind; but this case is not conclusive.

In the modern period, Dorcus suggested to a subject that certain real colours were some other colours, e.g. red was said to be green. In all cases the after images reported were those appropriate to the real stimuli used rather than to the colour suggested. It is conceivable that real sensory processes suppress hallucinatory activity, so that this study does not constitute conclusive negative evidence.

The first extensive investigation of after images in the modern period is that of Erickson and Erickson (1938). Five college students previously used for hypnotic work were the subjects. Their associations to the words "red, green, blue, yellow, bright" were checked in the waking and trance states. Trance induction took 30–45 minutes. In trance S was shown in turn 16 blank white sheets which, E suggested, were brightly coloured. E was to name the first, S the second, E the third, and so on. S was told that the colours he would see and name would be different from those named by E. S's after-images to real colours were also obtained, and he was questioned to determine his knowledge of the laws of after-images. The results: There was no evidence of word association along complementary colour lines. Questioning of Ss was said to reveal ignorance of after-image processes. Ss gave negative after-images to real colours. For the hypnotic hallucination situation (p. 583) ... "The 4 subjects who hallucinated the specified colour invariably declared the succeeding sheet to be the appropriate complementary colour."

The authors explain the result in terms of association: a real stimulus leads to a perceptual process which in turn leads to an after-image; when the percept is revived as an image it again leads to an after-image. The following criticisms may be made: (i) the results of the word association tests are not consistent with their explanation; (ii) with college students it is difficult to ensure complete naivity about after-images, and the Ss being "trained" may have concealed knowledge from E (cf. 55); (iii) although S was allowed 1 to 2 minutes to "hallucinate" a colour specified by E, the latency of S's response to his own blank is not reported; this is relevant in that S may have had opportunity to look at other coloured objects in the test room and thereby discover their after-images (cf. 5); (iv) terms such as "bright red" are ambiguous, as

Binet and Fere pointed out; there are individual differences in after-images to a given real colour; both of these factors would lead one to expect variability among the subjects, yet uniformity of correct response is reported. All told, these observations suggest that a tenable explanation of the results is that the subjects responded in terms of a knowledge of complementary after images possessed in advance or obtained during the experiment.

A more extensive investigation by Hibler led to the conclusion that (p. 56) "there is no evidence that hallucinations produce any cerebral or sensory changes in the organism which cannot be explained in terms of mere verbal agreement, and cooperation with the experimenter." While his experiment has certain procedural advantages over the Ericksons' study, it also has several important weaknesses which Erickson (1941) has pointed out: (i) The adequacy of hypnosis is doubtful, the instructions being brief and direct and the whole procedure taking but five minutes. (ii) The subjects were aware of the laws of after-images. Hibler's reply, while pointing to weaknesses in the Erickson set up, does not rebut these criticisms.

Most recently, Rosenthal and Mele found positive results with four deep trance subjects. While their experiment differs in detail from the Erickson study, the general design was similar and the criticisms (ii), (iii), (iv) directed against the latter apply equally to the Rosenthal and Mele study. At this stage the "reality" of after-images to hallucinated colours cannot be said to have been established. Future experiments might concentrate on naive subjects such as children and be conducted in wholly achromatic settings.

Negative hallucinations for colour seem not to have been studied before Young's incidental test of the effect of suggested colour confusion on the Holmgren Woollens Test. The first experiment was that of Erickson (1939). He used four female and two male subjects all of whom had been used regularly in hypnotic work. The subjects had "normal" colour vision on the Ishihara Test. The procedure was as follows: trance induction; suggestion of complete colour blindness; awakening of S with post trance blindness so that he would experience distress; reinduction of trance and partial restoration of vision; induction of amnesia for ideas and connotations of the negatively hallucinated objects; performance of the Ishihara separately under conditions of red, green, red-green, and total blindness; performance on the Ishihara without suggested blindness in the trance and waking states. The results were: (i) Ss had "normal" colour vision on the Ishihara in the "waking" and "trance" states when no blindness was suggested. (ii) When colour blindness was sug-

gested, the Ss were ... "unable to see" the numerals for which they were "blind". Erickson concluded that ... "the findings demonstrate clearly that cortical processes and activities can effect results similar if not identical with those based primarily upon peripheral activities and conditions."

A number of considerations, however, raise doubt that Ss "experienced" colour blindness, and an alternative explanation of their responses may be sought. Grether points out that Erickson's conception of colour blindness is mistaken. Ishihara's "red", "green", and "red-green" blindness should not be taken literally. Red-green blindness means, not blind for red and green, but confusing red and green; and the Ishihara "types" refer to theories of receptor deficiency in the 3-colour theory of vision. As Grether says (p. 208): "In both types of red-green blindness the actual deficiencies in colour sensation are quite different from those Erickson attempted to induce in his subject." Accordingly he asked "How then can we account for the fact that many of the hypnotically produced colour blind subjects read the chart in the same manner as do subjects with true red-green blindness?" In reply, he says that because of the way the Ishihara plates are made they would not discriminate the orthodox Ishihara types from Erickson's pseudo-types. Grether was not concerned with the "genuineness" of the sensory content, but he wished to show that Erickson, if he achieved any effect, did not produce colour vision anomalies of the sort that occur naturally.

Erickson's subjects were said not to be able to simulate colour blindness in the waking state. However, normal subjects can with scrutiny and certainly with practice come to pick the anomalous responses. (Erickson's subjects performed the test 13 times in all.) The need for independent normal controls for this experiment is obvious. Erickson gives no data on the rate and duration of exposure of the Ishihara plates, and no data on the latency of S's responses. It is possible that the responses were achived by deducing the anomalous response rather than actually "seeing" in the manner of a colour blind person.

Although Harriman (1942a) says he wishes to check on the validity of Grether's comments on Erickson's work, he commits the same error of taking the word "blind" literally. He follows Erickson's procedure in detail, finds the same sorts of result, and is open to the same criticism. Subsequently, in line with Grether's comment, Harriman (1942b) repeated the experiment with ... "instructions appropriate to induce colour vision anomalies", viz. psuedo-protanopia and pseudo-deuteranopia. He does not describe the detail. In this case he tested with the American Optical Company's Holmgren Colour Test and the Pseudo-Isochromatic

Plates. Apart from suppressing red and green, Ss were not able to perform in a way similar to the truly colour blind; and accordingly Harriman concluded ... "it is possible to induce a profound change in S's attitudinal behaviour, although it appears doubtful whether sensory content is actually changed."

In summary: Repeatedly, studies of positive and negative colour vision hallucinations have produced results which have been said to support the notion of real changes in sensory process. Methodological weaknesses in these studies, however, allow for reinterpretation of the results in terms of simulation. The status of after-images to hallucinated colours is still in doubt, although the form which an appropriate experiment might take is now clearer. Harriman's second study on colour vision anomalies goes against the notion of genuine colour blindness. In general then, while there are loose ends, this body of work is consistent with the "sceptical" point of view, while the occasional unambiguous result goes against the "credulous" view.

Temporal

Early studies concerned the subject's appreciation of time in hypnosis. More recently attention has been given to the suggested distortion of time and its effect on various performances.

As subject's could carry out post-hypnotic suggestions at specified times, it was at first thought that hypnosis improved a subject's judgement of time. It was seen that some suggestions specified a readily recognizable date, so then arbitrary intervals of time would be chosen: "perform X, 2559 minutes from now".

Delboeuf, Bramwell and Hooper used this method with lengthy time intervals, Gurney and Mitchell with short. Clausen has recently shown that this method, where E specifies an interval which S must match, is the most reliable of three commonly used; but in these earlier studies it had certain difficulties as Mitchell's findings showed. Although investigators claimed that their subjects had no knowledge of arithmetic, and the subjects when awake disclaimed calculation, enquiry showed that both claims were false. Subjects in fact used the following methods: S would convert a long time interval into units of days, hours and minutes and thereby fix the date and time for the execution of the post-hypnotic act. With short intervals, in the absence of clocks, S would use rhythmic counting, pulse or breathing as a basis of judgement. In an experimental study, Stalnaker and Richardson showed that subjects used counting, pulse, etc. "with astonishing success" for judging short intervals; but when specifically directed not to use such means, the subjects were no

more successful with hypnotic suggestion for improved time judgement than they were in the normal waking state.

More hallucinatory in character are the "time distortions". A subject in trance for about one hour may declare on awakening that he has been asleep for five minutes; this may be a function of post-hypnotic amnesia. In the trance itself, the passage of time may appear longer or shorter than it actually is (Welch) much as in the waking state where interest and boredom seem to affect it. In these terms, Cooper and Erickson studied time distortion produced by hypnotic suggestion.

Cooper and Erickson use a novel terminology which parallels a conventional distinction between "real" and "phenomenal" objects of perception. Generally, "real" time is called "world" time or WT, and "phenomenal" time is called "seeming duration" or SD. In an experimental context the real time fixed by E but unknown to S for a given task is called the "allotted time" or AT, while its phenomenal time—the time seemingly available—is called the "suggested personal time" or SPT. Activities in time are said to be either "continuous" such as breathing which is a process with no well defined end, or "completed" such as an intake of breath which has a discrete character. Time distortion is said to exist where WT $\neq$ SD for a given activity, i.e. when S falsely perceives the duration of an activity. Ordinarily the SPT as stated is not equal to the AT, but in the experimental setting the subject is required to misperceive the AT as equal to the SPT.

Cooper and Erickson investigated the "utilization of subjective time". While these authors espouse a philosophic idealism in adopting such a notion, their procedures can be recast into realistic terms. Their line of argument runs: "If SD > WT, perhaps one can get more done in SD than in WT." More consistently they might have argued that: "If SD > WT, perhaps one can seem to get more done in SD than in WT." Their experimental evidence can be brought to bear on both; all of their studies bear on the second, some on the first, depending upon the "activity" to be carried out in "time". In some studies, S's "improvement" is evaluated only by reference to an "imaginary" performance, and so they bear only on the second line of argument. In other cases evaluation is based on "actual" performance and these then bear on the first line of argument. Now, "utilizing subjective time" means in realist terms "engaging in an activity while falsely perceiving real time". If one compares actual and imaginary activities in relation to true and false perception of time, four situations arise, and performances in each case can be evaluated in one of two ways. These possibilities are set out below in tabular form to show the division amongst the Cooper and Erickson studies:

Time	Activity	
	S actually performs X	S imagines performing X
S correctly perceives Y	Control condition	Control condition
S falsely perceives Y as Z	Mathematical problems Nonsense learning Creative activities	Counting experience Human relations problems Rehearsal of motor skills

X is a given activity; Y is real time; Z is the SPT; change in performance of X as a function of a given time experience may be evaluated by reference to either (i) S's subsequent actual performance of X; or (ii) S's subsequent imaginary performance of X. An examination of Cooper and Erickson's work in these terms reveals that they do not always provide the appropriate controls, nor do they in all cases choose the appropriate means of evaluation.

Fourteen subjects were used, of varying age, sex and background, but not all in any one study which might involve but a single case. Subjects capable of deep trance were "trained" for the experiments, the crucial feature being acceptance of the suggestions of time distortion. Typically this involved agreement that 10 seconds seemed like 10 minutes. In evaluating the generality of the results it should be noted that subsequent to the original selection of subjects, the authors decided . . . "to continue training only with those who are promising." Typical procedures were as follows:

Time	Activity	
	Continuous	Completed
WT	"You will go walking."	"You will prepare a meal."
SPT	"You will spend at least 10 minutes walking."	"You will prepare a meal; it will take 1 hour."

The tasks performed in trance were (i) "contemplation" of experiences such as walking, swimming, shopping, etc. either as an experience to be described for its own sake, or as a possible means of learning as in the "rehearsal" of musical performance and handwriting; and (ii) "directed thinking" as in the solution of problems. A subject might carry out a number of tasks in trance before reporting on them upon awakening. The authors pay no attention to the possible influence of retroactive inhibition and post-hypnotic amnesia; the latter omission is especially puzzling as the subjects are presumably somnambules.

The results of particular studies may now be examined, first the imaginary and then the actual activities.

1. *Imaginary activities*

i. *Counting.* A typical study involved a subject "hallucinating" an experience on a farm where she counted strokes of a butter churn for SPT = 10 minutes, AT = 3 seconds. She reported counting 114 strokes in leisurely fashion, amongst other details of her experience. This would support the conclusion that S can seem to do more if she seems to have more time; but there is a paradox if one takes the subject's testimony literally. E tells S that 10 minutes is available, but stops her after 3 seconds. As S is presumably unaware of the 3 seconds limit on AT she might be expected to proceed at the rate "normal" for a projected 10 minute activity and thence to have completed 3 seconds of that activity at the normal rate when the signal to stop is given. Thus for 10 minutes of activity to have been experienced there would need to be a tremendous speeding up between the onset of the stop signal and the beginning of the next step in the trance sequence. It seems more likely that these reports are after-elaborations of a train of associations set off in trance—much as occurs in the reporting of dreams. No controls for "waking" performance are provided. When this performance is evaluated in relation to a subsequent actual counting performance further difficulties of interpretation arise. When awake, S illustrated her trance counting with strokes at the rate of 1 per second. If she had in fact counted at this rate, she would have reported 3 strokes for the AT or 600 strokes for the SPT; as she reported 114, clearly she did not count at the rate of 1 per second in trance, although this may have been the appearance to her. Suppose on the other hand that she did count 114 strokes in trance; then her rate would have been 114 per 3 seconds AT which is 38 strokes per second. There is no waking control on whether S could count at such a rate, but it seems most unlikely.

Other experiments on counting give similar results and it may be concluded: (i) S does not in fact count one by one in trance the objects described at the rate demonstrated when awake; it is unlikely in fact that S counts at all. (ii) On S's testimony, she seems to count and the experience is "real to her".

ii. *Human Relations Problems.* In a typical case the experimenter sets a woman subject the problem of giving advice to a young couple who wish to marry where the girl has a dependent invalid mother. The conditions were SPT = 10 minutes, AT = 10 seconds. The subject gave a detailed account of an imaginary interview with the clients and a solution. No waking control was provided on the "solution" of such prob-

lems. As the subject was 36 years old, she would have had her own and also vicarious experience in such matters from which to formulate a solution. Once again there is no objective evidence that S's mental processes were speeded up, although the subject may have had this impression. The same sort of comment can be made about Cooper and Erickson's dress designing problem.

iii. *Rehearsal of Motor Skills.* In this study, 5 subjects were to hallucinate writing with the non-preferred hand. Samples of actual handwriting were taken to gauge progress with "practice" in "distorted time". A waking control group which did as much writing as the experimental group performed as well at each stage although it had no rehearsal in fantasy. Once again there is no evidence for real effects on behaviour, although the subjects reported an impression of improvement.

2. *Actual Activities*

i. *Nonsense syllable learning.* One subject learned nonsense material in the waking state and again in trance with time distortion. Learning was by the method of paired associates. The average time for learning a letter-group-pair was shorter with time distortion; but there was no difference between the trance and waking conditions for measures of retention. Insufficient data are given to enable evaluation of these results, e.g. there are reported no item data which would show that the two paired-associates lists used were of comparable difficulty. This experiment would need repetition with independent waking controls and more subjects.

ii. *Mathematical problems.* Two subjects performed simple arithmetic problems in the waking state and in trance with time distortion. When no allotted time was set, the subjects did no better than in the waking state. When the subjects were allotted time such that WT < SPT... "the performance in terms of correct answers was no better than chance would give." Though not explicitly stated, it appears that the Ss were hardly able to begin a problem before their allotted time was up, and they could then do no better than guess.

In summary: The uniform outcome of these studies and others not discussed here, (with the dubious exception of the study of nonsense syllable learning), is that no *real* effect upon behaviour is induced by time distortion. The subjects *did not* count, solve problems, benefit from imaginary practice, or the like; although they behaved in their testimony as if they did. The "credulous" view is not supported by these studies.

Studies of Regression

A common device in early demonstrations of hypnosis was to suggest to a subject that he was some other person, or in some other time or place.

Binet gives priority to Bourru and Burot (1887) in requiring S to relive part of his past life. After working with the French investigators, Freud with Breuer developed the method of abreaction. The term "hypnotic regression" has been used generally to refer to all forms of behaviour manifested by the subject in response to the hypnotic suggestion that he is younger. Erickson and Kubie introduced a distinction between regression and revivification, the former "a half-conscious dramatization of the present understanding of that previous time" and the latter that "type of time regression in which the hypnotic situation itself ceases and the subject is plunged directly into the chronological past." Gill rejects the latter outright in light of the continuing rapport between subject and hypnotist. Weitzenhoffer (1953) distinguishes three types of regression as behaviour, and revivification as the process whereby such behaviour is brought about. The first type of regression, RI, refers to dramatizing or acting out the past; RII refers to "a true psycho-physiological return to a past state"; and RIII is a blend of the first two. The "sceptical" view is that only RI occurs, while the "credulous" view is that RII can sometimes occur. Experimental work has concerned the developmental aspects of mental test performance and physiology.

1. *Mental Test Performances*

Young (1926) regressed a subject with IQ = 140 to the age of 4 years, and also had him "play the game of being four years old" in the waking state. Tested under these two conditions the subject gained IQs of 183 and 189 respectively. Young "believed that at least in this case the S, all unconsciously and with great verisimilitude, was playing a role" and he rejected the notion of RII on the grounds later given by Gill.

Platonow carried out a similar study, although there is some question as to whether his subjects were in fact hypnotized. He claims positive results for regression to 6 years of age; but as Young points out his results are suspect on several counts: the actual IQ of his subjects was not determined; there was no test of waking controls to see whether they might perform in the same way as the trance subjects. Young (1940) followed up the early studies with 14 somnambules and 7 non-somnambules. In the first study, 10 Ss were hypnotized in a group setting, rapport was transferred to 10 mental testers for the determination of regressed IQ. The actual average IQ of the group was around 100, but in regression to the third birthday, the results showed an average excess of 18 months MA over the regressed CA. He further tested individually 9 trance subjects and 7 non-hypnotizable subjects, the first group hypnotically regressed to 3 years of age, the second simulating an age of 3 years in the waking state. The results for the groups were: actual IQ =

100; hypnotically regressed IQ = 198; waking simulated IQ = 178. This showed that neither group could produce the actual performance of three year olds.

Spiegel et al. used an elaborate procedure for regressing one subject to 2 years of age and then progressing step by step to his proper age. The Stanford-Binet test was given at each stage. With a real age of 23 years, the subjects IQ was assessed at 123; regressed IQs varied from 95 to 134. The manner of testing at each level was not described: it could make a difference to the results if the same or a different basal item level was used at different testings. Their waking control tests were not comparable to the trance test: "S was asked a random series of Binet questions and was to respond 'as if you are five years old' ... etc." Perhaps their most significant finding was that when 18 months later the subject was regressed to the day of testing and retested ... "the results were a score slightly higher than the original when awake, and his pattern of responses differ." No evidence of RII.

The latter study illustrates a control feature later emphasised by Sarbin. He had actual Binet test records for 12 subjects obtained when they were 8 or 9 years old. (The ages and IQs of the subjects current at the time of the experiment are not reported.) The subjects were regressed to the day of testing at 8 or 9; and separately they were asked to simulate in the waking state an age of 8 or 9. The subjects were unable to reproduce the original test pattern under either condition, though they were a little closer to it with regression than with simulation. Kline (1950, 1951) reports a series of studies which are said to demonstrate RII, but these lack certain necessary controls which render their interpretation ambiguous: no waking controls were used; there are no test records for the age to which the Ss were to be subsequently regressed.

Several studies have attempted to assess "personality" changes with hypnotic regression. Bergmann et al. and Mercer and Gibson each used one subject regressed to several levels after the manner of Spiegel et al. and tested at each stage with the Rorschach. Mercer and Gibson also used the Goodenough Draw-a-Man Tests and the Stanford-Binet Vocabulary. These investigators claimed positive results in that the Ss' behaviours compared favourably with the test norms for the age levels chosen. These studies, however, are inconclusive in that no waking controls were provided, and no test records were available for the age to which the Ss were to be subsequently regressed. Orne's (1951) study meets these objections. He regressed 10 subjects to their 6th birthdays and found that Rorschach records, handwriting and drawing were unlike the norms for 6 year olds. In the waking state, subjects imagining that they were 6 years old produced results similar to those found with

hypnotic regression. Rorschach protocols and drawings were available from the 6th birthday of one of Orne's subjects and these were different from the "regressed" test performances.

In general, studies of regression by mental tests are methodologically unsound and their conclusions are open to question. The few studies providing adequate controls refute the notion of RII. The overall picture favours the "sceptical" point of view.

2. *Physiological indeces*

A case commonly cited as evidence for RII is the apparent occurrence of unconsciousness in one of Erickson's patients at a point in a regression sequence at which (on independent evidence it was known) he had been hit on the head by thugs. According to Erickson (1937, p. 1286):

"The regression to the earlier period of life at which this experience originally occurred, occasioned by the hypnotic suggestions for reorientation, apparently functioned in such a fashion that all experiences subsequent to the event, including even the development of amnesia, were eliminated by hypnotic dissociation from the patterns of response which were manifested in the experimental situation. Thereby revival of the experience with its associated responses was permitted as if it were the course of actual development. Once this process had been initiated by the evocation of readily accessible memories, the recovery of each item functioned in itself as an aid in recovering additional material in its original chronological order, thus constituting a continuous progression to completion of the sequential activity. Hence there would be aroused in proper order and relationship the concomitant psychic and somatic activities with their corresponding alterations and adjustments of the mental and physical states, the entire process being directed by the originally established patterns of response."

This is held to be a genuine case of RII, but the following reservations should be noted: (i) Independent evidence shows that while the regression session was only 4 hours long, the events reported extended over some days. The "unconsciousness" in trance lasted several minutes, whereas in the original situation, S did not recover full consciousness for many hours. Thus as the temporal characteristics of the real and regression situations differ, they must also differ in their behavioural details: the regression is not like the rerun of a sound film. (ii) Despite Erickson's (LeCron) contentions that the hypnotist must adopt a role pertinent to the S's regressed state, his role in this case is not specified; and to judge from the questions he put to the subject (p. 1283)—"What's the matter? What's happening?"—it seems unlikely that he

was playing the part of one of the thugs. (iii) The regression sequence was repeated after more than one year, and again a further five months later; and (p. 1284) ... "the records obtained were compared with those of the first investigation and were found to be essentially identical, including all the findings contained in this report. The same descriptive details, the same sequences, the same physical manifestations and even the same fragmentary utterances and gaps in the re-experiencing of the events were found." The verbatim records are not reported so that an independent assessment of the extent of identity cannot be made. The claim of identity from session to session appears to be an overstatement, for, considering the part he played in the regression sessions, Erickson himself would have had to repeat exactly in the right sequence his suggestions, questions, comments, etc. Accordingly this report cannot be accepted as conclusive evidence for RII.

Another case report is provided by Kupper. A patient, diagnosis doubtful, suspected of malingering, in 1940 showed "diffuse abnormality" on EEG and in 1943 showed "diffuse cortical irregularities consistent with a convlusive disorder". It is not stated whether this was an occasional or continuing pattern. When the subject was regressed under hypnosis to a traumatic occurence in 1938, certain changes in the EEG were noted (p. 17): "He was taken back to his 12th birthday and the EEG was within normal limits. It remained so through succeeding suggested years until his eighteenth year in 1938 following the first attack. Then diffuse cortical abnormalities occurred. By suggestion the patient was placed in the situation prior to the initial attack on the fatal morning. More irregularities, abnormalities and spiking occurred. The record was then changed within normal limits by reassuring the patient." No data or records are provided, no waking control (with mention of the traumatic incident) was used. It is possible that S's EEG train was only incidentally abnormal in relation to memories of the traumatic incident.

Gidro-Frank and Bowersbuch studied the plantar response under hypnotic regression. In 3 of an original 45 subjects "dorsiflection of the great toe and fanning of the other toes" was found when the subjects were regressed to 5–6 months of age. While plantar flexion was noted at other ages, no quantitative data are given so that it cannot be determined whether these were invariable reactions. There were no waking controls—a necessity in light of other evidence (Weitzenhoffer, 1953) that reversal of the plantar reflex is not uncommon in adults.

The authors note that their subjects' muscular coordination remained adult despite their regression to a very young age, and they continued to understand and communicate with the hypnotist. These findings suggest RI rather than RII.

True claimed to have devised a simple method for distinguishing RI and RII. This requires S to give the "day of the week" on which a given event occurred, such as Xmas Day or S's birthday. He questioned 40 men and 10 women about recent events and found "poor memory"; while in hypnotic regression 82% were correct in stating the days of their 10th, 7th and 4th birthdays. No details are given of the ages of the subjects or of their knowledge of their most recent birthday. Time taken to give an answer is not reported. No waking controls were used. All of these facts are important in that True may have fallen into the same error as that made by the early investigators of time judgement (Bramwell, 1900, Delboeuf, Hooper). That is, without actually "reliving the day" through hypnotic regression, subjects could give the correct day by deduction from available information: 1 year = 52 weeks and one day, and 1 leap year = 52 weeks and 2 days; thus to specify the day of a given date one has only to count back from 0 to 7 days as the number of years specifies.

In summary: The study of hypnotic regression is in many cases as backward as the investigations of the 19th century. Repeatedly crucial controls are lacking and evidence is anecdotal. The few methodologically sound studies refute the notion of RII. In the absence of rationale of the link between hypnotic procedures and alleged hypnotic effects, RII is as magical a notion as many of an earlier period and only one step removed from its popular version which involves reincarnation (Cf. the case of Bridey Murphy). The most significant need in future studies is for objective records of the subject's behaviour at time t_1 so that subsequently at time t_{1+x} when S is regressed (or is required to simulate regression) through the time interval x the two behaviours can be compared. The weight of evidence to date supports the "sceptical" view that the subject's behaviour in these situations is a form of role-playing.

Studies of Anti-Social Behaviour

The French Commission of Enquiry into Animal Magnetism (1784) in a private report discussed the possible use of magnetism in sexual seduction (Binet, 1887, p. 18–25), and it was of the opinion that "such practices and assemblies may have an injurious effect upon morality." The possibility of using hypnotic suggestion for the production of other forms of "anti-social" behaviour such as theft was investigated by Braid (Bramwell, 1906). Bernheim (p. 160) reports a series of cases of false testimony induced by indirect methods such as the use of retroactive hallucinations and delusions. Bramwell (1906, p. 323), having questioned hypnotized subjects about their behaviour, described "anti-social" acts as "a palpable farce". He quotes de Jong with approval

as saying that the subjects will resist certain disagreeable suggestions ... "while they execute others alleged to be criminal, because they recognized that these were laboratory experiments devoid of danger either to themselves or others." Schilder and Kauder (p. 52) in similar vein argued ... "that the hypnotized person is always aware of the general situation, that he is conscious of the fact that an experiment is being made upon him, and that he must be well aware that the hypnotizer is not inducing him to commit an actual murder, if the hypnotizer is a man of respected social position. We are therefore of the opinion that the hypnotized person can only be brought to perform such crimes as correspond with a previously existing inclination on his part." Erickson (1939), as Young (LeCron) points out, is somewhat out of character in supporting this view. He describes 35 widely differing circumstances in which he was unable to induce anti-social or self-injurious behaviour. His particular definition of "anti-social" involves awareness on the part of the subject of the reprehensible character of the behaviour suggested. This committed him to the use of direct suggestion rather than indirect methods through hallucination and delusion and the role of "demand characteristics" (Orne, 1959) in determining his results would need to be carefully considered.

If one adopts a legalistic definition of an "anti-social act", e.g. theft = taking another person's property without legal right, then the weight of evidence supports the view that hypnotized subjects may be induced in particular settings to behave in anti-social ways. Wells and Brenman successfully induced theft and breach of confidence. Schneck (1947) and Watkins (1947) report a variety of military offences. Rowland (1939) and Young (LeCron) report attempts at injury of self and others. Weitzenhoffer (1949) has argued that the difference in outcome, between the studies of Erickson on the one hand, and of the other investigators on the other, is probably a function of method. That is, if one can make use of indirect methods to delude the subject it may be possible to induce anti-social behaviour; whereas the subject is reluctant to so behave if it is made clear to him that his behaviour is morally reprehensible. Lyon's study gives some support to this view in that anti-social acts were more readily induced when justification and rationalization was given for an act than when S was simply commanded to act.

Some of these studies provide no waking controls for S's behaviour, a minimum requirement for demonstrating the necessity of hypnotic suggestion; and of those which do one may ask whether the control is an appropriate one. de Jong, Schilder and Kauder, and Erickson all comment on the setting in which the study is carried out; and repeatedly one can point to features of experiments which would be recognized by

the subject as "staged". For instance: in Lyon's experiment why leave a confidential report in an open and unattended office? in Rowland's and Young's experiments why expose only E's head as a target? The experiment would need to be conducted in "non-staged" circumstances; and the appropriate waking control would then be S's behaviour while awake in a comparable setting.

In summary: There is disagreement over the definition of anti-social behaviour and the methods of inducing it, but most studies report that subjects will carry out suggestions given to them. If the subject's testimony can be taken as relevant, it points to compliance with the demands of a person in a position of trust: (Lyon, p. 290) "I really didn't want to but I believed you"; "I thought it would hurt him but not seriously; but I thought this must be pretty important to you so I took a chance"; "Well you said it was alright to do it". This would support the "sceptical" view of the subject's behaviour; but this should be tempered by the fact that an adequate test of the "credulous" view has yet to be made.

General conclusions: More often than not studies which are held to provide evidence for the "credulous" point of view prove to be inconclusive. The occasional well designed study tends to refute the "credulous" view; and so all told the evidence can be said to directly support or be consistent with the "sceptical" point of view. If this were a debate, one might say that the "sceptical" view wins by default; but as the scientist is less concerned to win arguments than to discover truth, the case cannot be let rest. There is a need made clear by this review for more rigorous methodology: many studies need to be redone with the proper controls to enable unequivocal conclusions. As a basis for evaluating current work and for planning future studies, the comparisons and controls discussed an illustrated throughout this review may be brought together into a single tabular statement of the basic design for hypnotic experimentation:

Basic Design for Experiments on Hypnosis

Major comparisons	Stimuli	Hynotizability	
		Somnambules	Non-somnambules
Control	Present		
	Absent		
Hypnotic trance	Present		
	Absent		
Hypnotic trance plus suggestions	Present		
	Absent		
Waking simulation	Present		
	Absent		

Replication: i. Independent random samples of subjects for the 16 conditions; ii. Independent hypnotists for the 8 hypnotic conditions.

The information provided by this design is as follows: The "control" condition provides a baseline of performance in the "presence" and "absence" of certain stimuli, e.g. how does the normal waking subject react in the GSR setup to (i) pain; (ii) the absence of pain. Against this baseline one can determine whether trace induction procedures alone make any difference; whether trace with experimental suggestions makes any difference; and whether simulation has any effect. Furthermore one can make internal comparisons to see whether, for example, hypnotically produced behaviour differs from simulation. Presence and absence of stimuli are included as conditions to enable the study of both positive and negative hypnotic phenomena, e.g. positive and negative hallucinations; but also as a control on the experimental device being used—if the subjects react indifferently to the presence and absence of stimuli one's measures are either unreliable or irrelevant—and to provide the appropriate baselines for evaluating the effects of hypnosis. Two degrees of hypnotizability are included to allow for differential reaction to trance suggestions and thereby, with the somnambules, provide favourable conditions for the test of the "credulous" view. The use of independent groups of subjects will offset the difficulties raised by Pattie (1933), and repetition with independent hypnotists enables the evaluation to certain "demand characteristics" (Orne, 1959). The factorial design also enables assessment of interactions amongst the main variables. The design is general in application and particular experiments will need additional controls, but its use can be expected to increase the likelihood of conclusive experimentation on hypnosis.

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HYPNOSIS IN THE MANAGEMENT OF A CASE OF *PLACENTA ABRUPTIO**

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Hypnosis has been used in various degrees for a wide variety of disorders. Symptom substitution, transformation, and eventual removal often is effective therapy. Hypnosis is utilized also as a diagnostic and psychotherapeutic tool. (See, for example, Raginsky.)

Specifically, considerable work has been done in the management of pregnancy and delivery with the aid of hypnotic therapy, among others notably by Kroger (for example 1953, 1946).

In obstetrics, there are reports on the use of hypnosis for nausea and vomiting (Kroger, 1946), habitual abortion (Raginsky), pre-eclampsia toxemia (Ambrose); nevertheless, there appears to be relatively little information on specific symptom control and management in a difficult pregnancy.

The present case may serve as a guide toward further uses for this tool and as basis for further investigation.

As with so much material in this area, this case, by its very nature, must be presented largely "anecdotally"; there is of course no scientific control, merely the clinical judgment of the authors.

Briefly, the basis for such judgment is that Rawlins is a medical practitioner, specializing in obstetrics, with an experience of well over two thousand cases. The present case was in her charge. Her approach is holistic. Her practice utilizes Read's method of "natural childbirth" (1944) extensively. She does not herself practise hypnosis.

Hartmann is a teaching, counseling, and research psychologist, operating within a phenomenological orientation. He had only a theoretical knowledge of hypnosis at the beginning of this case.

* Dr. William S. Kroger's critical review of this paper is gratefully acknowledged.

The subject, 37 years old, was a mother of two. Both these children had been born by the Read method, with no complications whatever. Despite the desire to nurse, breast feeding in both cases had been difficult, brief, and never without supplement. The subject's history indicates a marked degree of freedom from neurotic symptoms.

Fairly constant, uncomfortable, though not severe nausea was experienced in the first and second pregnancies from about the third to the 14th week of gestation. In the second pregnancy, this was alleviated for several hours after each of several inductions of hypnosis. The hypnotherapist was Glasner (1955).

Almost immediately after the start of the third pregnancy, the one under consideration, more or less constant spotting and frequent uterine hemorrhages ensued. Examination revealed no obvious pathology; on the contrary, the subject was in excellent health. Uterine contractions occurred, with gradually increasing intensity, experienced as "cramps." In addition, general pressures and aches in the abdominal area, backaches after brief periods of standing, headaches, and a general feeling of strong discomfort and illness persisted.

Hormone treatment (Delalutin, 250 mg. every three days, from the 8th to the 14th week) failed to alleviate the bleeding and was discontinued after an allergic reaction. Bed rest was of little or no help. (In the context of this symptomatology, nausea was a relatively minor additional discomfort; Compazine was used to control it, from about the fifth to the eighth week. It was then used very little, because of too great consequent lassitude.)

The differential diagnosis considered decidual bleeding, *placenta previa*, and *placenta abruptio*. In view of the severity of the uterine hemorrhages and the strong contractions, prognosis for a live baby was poor.

However, A-Z tests made after particularly profuse hemorrhages in the 12th and the 16th week of gestation were positive, suggesting a still surviving fetus.

Kroger was called in at the end of the fourth month. With prognosis poor and symptoms out of control, it was felt that hypnosis might help by alleviating psychosomatic tensions which were exacerbating the situation.

At a stage when uterine hemorrhages and contractions had been quite severe and the subject was in considerable discomfort, Kroger readily induced hypnosis. His suggestions constituted primarily training in relaxation; further, typically for Kroger's (and others') work, they were directed in a permissive manner to enhance subject's feelings of

confidence and of "being in control." Auto-hypnosis was taught, and Hartmann was placed "en rapport" with the subject.

Bleeding and contractions diminished considerably and all other symptoms disappeared, for over 48 hours. These gradually reappeared, but were considerably less than had been the rule, for about four days. Subject practised auto-hypnosis, and Hartmann re-enforced the suggestions given by Kroger.

Then, during the 18th week of gestation, a severe hemorrhage occurred, accompanied by strong uterine contractions. A sizable blood clot came away. Abortion had not occurred, but appeared imminent. Kroger was unavailable. Another medical practitioner experienced in hypnotism induced "deep" hypnosis. The bleeding and contractions subsided, and all pain and discomfort disappeared.

Following another hypnotic induction by Kroger, the subject achieved a state of complete physical and mental well-being. Subsequently, Hartmann took over, re-enforcing and widening suggestions, "deepening" the hypnotic state, and increasing the subject's ability in auto-hypnosis.

From now on, hypnosis was induced every day, usually more than once. Bleeding decreased to relatively minor spotting; contractions were hardly more than normal for the fifth month of gestation. Feelings of discomfort were sharply reduced in frequency and intensity, and disappeared entirely after hypnosis of usually about ten minutes' duration. Deep, natural sleep and periods of invigorating, relaxing rest could be obtained at will through hypnotic suggestions.

About the 29th week, the uterine contractions became strong and regular, about ten to twenty minutes apart. Hypnosis was induced, and, as usual, discomfort disappeared and rest was easily obtained. The contractions subsided, but returned in 24 hours; procedure was repeated with the same results. In the next 24 hours, the uterine contractions gradually became stronger and more frequent.

Hartmann was constantly available to the subject throughout these months, because a close personal relationship existed between them. It is perhaps interesting that neither the subject nor Hartmann—trained and experienced as they were—admitted the possibility of labor having started. Determined, as it were, that this pregnancy was to be carried to term, they steadfastly refused to admit the obvious. Thus, while in contact with Rawlins, Hartmann did not inform her of anything out of the ordinary. This is additional confirmation of the danger and difficulty inherent in someone's treating others with whom he is personally involved.

Hypnotic suggestions were directed at inducing relaxation, feelings

of comfort, and cessation of contractions. Although ineffective for the latter, the approximately five hours of labor were characterized by little or no discomfort.

At what we now know was the beginning of the second stage of labor, it became clear that "hypnosis was not working." Rawlins was called, realized what was going on, and ordered immediate hospitalization.

Brought to the hospital by ambulance, the subject was transferred at once to the delivery room, where within minutes a 2 lbs. 6½ ozs. male was delivered in the unruptured amniotic sac, followed immediately by the intact placenta. Examination of the placenta revealed evidence of an old separation covering about three-fourths of the maternal surface.

The infant exhibited surprising vigor from the start, which helped him through some complications (anoxia, incipient hydrocephalus). Contrary to expectations in "premies," the infant has exhibited very strong resistance to infection. Weight gain was highly satisfactory, and became excellent after blood transfusion. At 12 months, no abnormality is detectable.

In Rawlins' experience, no other case of *placenta abruptio* with this kind of symptomatology, carried to only 29 weeks, has survived. The new variable here introduced appears to have been hypnosis, used to induce relaxation and psychosomatic comfort.

We have no doubt that hypnosis was, if not the only, then a major factor in bringing about the conditions under which the fetus was, despite these adverse circumstances, carried at least to this stage. Clearly, even ounces or days less would have reduced the chances of survival disastrously.

The following remains to be stated.

Parturition was remarkably easy, even for such a premature infant, and took place under most favorable conditions. The mother, chemo-anasthesized briefly, made a surprisingly fast recovery to complete health and vigor. It seems likely that hypnotic procedures account, at least in part, also for these additional advantages.

The infant was breast-fed completely from the beginning to the sixth month—this despite the previous lack of success, the continuing tensions and worries about the baby, and the necessity of using a breast-pump for the first two months when the child was in hospital; there was not even the frequently observed let-down, which occurs in so many cases of much less strain: both hetero- and auto-hypnosis continued to be used extensively to bring about the relaxation necessary for this undertaking.

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THE SYSTEMATIC USE OF HYPNOSIS IN INDIVIDUAL AND GROUP PSYCHOTHERAPY*

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Since the days of Mesmer, who was not even aware of the psychological nature of hypnosis, its use for therapeutic purposes has undergone a variety of metamorphoses. Mesmer used it as a cure-all. Later, it was primarily used for anesthetic and analgesic purposes (Elliotson, Ward, Esdaile). During that period, the psychological nature of hypnosis was recognized by Braid. During the last half of the nineteenth century, hypnosis was extensively used for the removal of symptoms by direct suggestions. While Charcot conceptualized hypnosis as a pathological hysterical phenomenon, the Nancy School (Liebeault, Bernheim) demonstrated that hypnosis was a normal psychological state. Janet, conceptualizing psychopathology as dissociated consciousness, gave two new directions to the therapeutic use of hypnosis. He used it to recover painful dissociated memories and to re-integrate them into awareness. Morton Prince followed in his footsteps. (Boring, 1950; Brenman and Gill, 1947; Dorcus, 1945; Jenness, 1944). Breuer stumbled accidentally upon the same discovery and correctly placed the emphasis on the catharsis of the dissociated affects. Freud recognized the full significance of Breuer's discovery, pointed out that dissociation is not a passive process, but is actively brought about by the repression of unacceptable impulses and ideas, and a new, dynamic conceptualization of psychopathology was born (Freud, 1952a). Freud now used hypnosis to uncover a symptom's motivational causes and to abreact the repressed affects. Freud abondoned the use of hypnosis because there were many patients whom he could not hypnotize to the required depth (Brill, 1938; Freud, 1952b). Later, he rejected hypnosis because he felt that by its use resistances and transferences were by-passed rather than analysed (Freud, 1953a, 1953b). The further development of hypnosis as a therapeutic tool came to a relative standstill until Lindner (1944), Wolberg (1945), Brenman and Gill (1947), and others, combined psychoanalysis and hypnosis in the hypnoanalytic technique. Here, the

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hypnotic trance is used when free association is stopped by resistance, in order to get at the resistance; to convince the patient of the reality of psychological conflicts and defense mechanisms by hypnotic implantation of artificial conflicts; to coax the patient to give up his repressed secrets by special devices, such as, automatic writing and crystal gazing; to recover traumatic events; to bring about the disuse of motiveless habit patterns and to strengthen newly acquired habit patterns.

More extensive therapeutic use of hypnosis was also inhibited by the uncritical acceptance of a number of shibboleths, of which the following are the most important:

1. Only few people are capable of reaching a somnambulistic trance. It becomes ever more evident that this is largely a matter of technique and time (LeCron, 1952).

2. The hypnotized patient is a passive, somnolent "zombie," without defenses or self-assertions. This, as was clearly pointed out by Wolberg (1945), is primarily the result of the operator's attitude. When the patient is given the freedom to move, act, and talk freely and spontaneously during deep trance, he will do so, and the casual observer will never know that the subject is hypnotized. Moreover, the subject never surrenders his defenses: they manifest themselves clearly even in deep trance. They are merely dealt with in a more economical manner because material is more accessible in the trance state.

3. Direct symptom removal is only temporary: relapse is the rule, or substitute symptoms make their appearance. This shibboleth maintains itself with remarkable tenacity, inspite of much evidence to the contrary, and, thus, prevented a thorough exploration of the question as to what kind of symptoms can safely be removed by direct suggestions and what kind require an uncovering technique. One should not minimize the role which technique plays in this problem: it makes quite a difference how a suggestion is worded and whether direct or indirect suggestion is used.

4. Direct symptom removal is possible only in deep trance. Actually, suggestions are effective when repeatedly given during light trance and, if necessary, by reinforcement at periodic intervals (Van Pelt, 1956).

In hypnoanalysis, hypnosis is used as an accessory technique. The author suggests that hypnosis can be used as an integral part of, and directing agent in, the therapeutic process, which he conceives as proceeding in an orderly fashion. This conception is based on the acceptance of psychoanalytic findings that a symptom or a character attitude has a definite structure (Fenichel, 1945; Reich, 1949).

It is the function of a symptom to bind anxiety and to protect the ego from the devastating effects of too much free-floating anxiety. Each symptom, like a dream, has two legs: one rests upon a conflict during

ontogenetic development, the other upon a conflict during adulthood, the latter of which may have precipitated the formation of the symptom. The symptom contains within itself the primary gain, the surreptitious expression of the unacceptable drive, and the primary defense, or the particular defense mechanism which prevents the break-through of the particular drive impulse into consciousness. Each symptom contains a number of derivatives of this original conflict. Finally, the symptom becomes integrated into the ego-structure, or ego-syntonic (Fenichel, 1945; Nunberg, 1948; Reich, 1949).

Ever since the publication of Reich's, by now classical, *Character-analysis*, it has become a more or less accepted dictum that analysis, to be successful, must proceed in a more or less orderly fashion, from the surface to the depth, or a chaotic analytic situation will develop, with more or less disastrous consequences for the patient (Fenichel, 1939; Reich, 1949). The author believes this to be also true for psychotherapy in general. Thus, it would be fruitless to confront the patient with the secondary gains which he derives from his symptoms, as long as the symptom is still ego-syntonic; or to uncover the ontogenetic leg of the symptom before its present-day leg has been uncovered; etc.

Hypnosis can be used with great advantage to give direction to this orderly process. First, it can be used to alienate the symptom by having the patient recall and/or regress to the many situations when the presence of the symptoms caused him distress and discomfort, and then to confront him with the many rationalizations by means of which the symptom became integrated into the ego-structure. After alienating the symptom, hypnosis can be used to uncover the secondary gains. This will liberate free-floating anxiety which, when followed closely, will lead to the present-day conflict. This process can be helped along by using hypnosis to locate the onset of the symptom and to unravel the precipiating events. In dealing with conflicts, present-day as well as ontogenetic, the ego-defensive aspects must be identified first. This, again, will liberate free-floating anxiety which, when followed, will lead to the identification of the drive impulses and which is facilitated by the trance state. Only when present-day drive impulses and affects are identified and abreacted may one proceed to uncover the ontogenetic leg, and this can be done by a hypnotic regression technique.

While, so far, nothing has been said about resistances and transferences, we have been dealing with them all along: an ego-defense manifests itself in therapy in the form of resistance and transference resistance. It should be noted that this technique neither by-passes nor undercuts resistances: the resistance and its underlying ego-defense is identified, uncovered, and followed in all its important ramifications.

Before therapy is terminated, the hypnotic relationship itself must be analyzed, just as any other positive transference.

Hypnosis may also be used to bring about the disuse of now motiveless habit patterns and to strengthen new and more appropriate ones.

This technique appears to be particularly applicable to states of hysterical disorders and psychosomatic conditions. Also, with patients who form only very tenuous relationships, are overly defensive, given to facile rationalizations, and, thus, need the additional bond of hypnotic relationship and the encouragement to face their problems under a condition where they feel they are less responsible for their thoughts and feelings than in a state of ordinary consciousness; this group includes alcoholics, addicts, other impulse-neurotics, and psychopaths.

Psychotherapy sessions should alternate between sessions at full awareness and hypnotic sessions. One should not proceed from one stage to the next until the goal of that particular stage has been fully integrated into consciousness. One must never use the hypnotic trance to force the patient to confront something consciously which he is not ready to deal with and accept, or the patient's ego integrity will be threatened by overwhelming and unmanageable anxiety. Ego integration takes time, differs from individual to individual, and the pacing must be largely left to the patient.

In dealing with psychosomatic conditions, it is often advisable or necessary to combine the uncovering with a direct-suggestion technique, particularly, when the symptom is serious, as, for example, a peptic ulcer which may perforate the stomach wall. In such cases, direct suggestions can be used advantageously, in conjunction with medication, to control pain, relieve spasms, and to bring about a temporary state of psychological quiescence which will provide optimal conditions for healing. In using such a technique, a number of important cautions must be observed, the discussion of which is beyond the scope of this paper.

For drug addicts, alcoholics, other impulse-neurotics, and psychopaths, group psychotherapy is, in the author's opinion, the therapy of choice, for reasons which have to be elucidated elsewhere. But even group psychotherapy is difficult because these patients tend to go on "verbal jags" about their ego-syntonic symptoms, are relatively fragile and overly defensive, tend to run from therapy as soon as they begin to experience some anxiety, and often form only very tenuous interpersonal relationships. For such patients, group hypnotherapy seems particularly indicated, inasmuch as they establish a stronger therapeutic bond and develop an increasing tolerance for anxiety. Moreover, the hypnotic inquiry is most effective in cutting through the "verbal jags" and in uncovering the psychological reasons for engaging in such "dry drunks."

Before starting hypnotherapy, and particularly group hypnotherapy, a state of deep trance must be firmly established and conditioned to a signal. Before starting induction procedures, the nature of hypnosis, and especially the patients' attitudes toward hypnosis, should be thoroughly discussed, in order to dispel apprehensions, deal with resistances to the use of hypnosis, and in order to subtly reinforce the patients' belief in the possibility and efficacy of the hypnotic trance. This preparatory discussion is an essential and integral part of the induction procedures; without it, many failures may be anticipated.

The induction of the trance is most effectively accomplished in the group setting because the group contagion may be used very effectively to build the belief in the efficacy and reality of the trance. It takes approximately two to three one-hour sessions to firmly establish a somnambulistic trance. No particular method of induction need be followed; on the contrary, familiarity with a variety of methods and the therapist's flexibility to shift from one method to another one seem to be important ingredients for success. By following Mayer's (1943) principle of fractionation, involving the utilization of the patients' own experiences in building the belief in, and thus deepen, the trance state, the latter can be induced efficaciously. Some cautions must be observed. One must not challenge a patient to resist a suggestion until one is certain that he is not able to successfully resists it; otherwise, the group contagion will operate in a negative rather than in a positive direction. It is advisable to have the patients shut their eyes during the induction procedure, so that they are not able to visually observe the other patients. Suggestions should be interspersed with comments which give the patients the impression that the suggestions are effectively carried out by the other patients.

Once the somnambulistic trance is firmly established and conditioned to a signal, a few minutes should be devoted at the beginning of each hypnotic session to reinforcing the trance state, or the trance will flatten out over a period of time.

Before starting with the hypnotherapy proper, the patients should be carefully instructed, during trance, that they well be able to remain in a deep trance state with their eyes open, and that they will be able to move, act, and talk freely and spontaneously. With impulse neurotics and psychopaths, one will also find it advantageous to begin the hypnotherapeutic sessions with an experimental demonstration, by means of a post-hypnotic suggestion, of the mechanism of rationalization, in order to point up the difference between rationalization and motivational causation. Such a demonstration is very convincing and seems to enhance the therapeutic process.

With alcoholics and drug addicts it is often necessary to combine

direct suggestions with the uncovering technique. The decrease and eventual disappearance of the desire for alcohol or drugs is suggested directly. Suggestion is also used to endow a harmless substitute with greater potential for gratification than the dangerous agent can provide. Such suggestions must be given repeatedly and must be reinforced throughout therapy.

Time is not available to discuss the cautions which must be observed in following such a technique. It should be clear, however, that no one should attempt it who is not thoroughly trained in psychotherapeutic techniques and who is not familiar with hypnotic techniques and phenomena.

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STATISTICAL REPORT ON GENERAL PRACTICE HYPNODONTICS; TAPE-RECORDER CONDITIONING*

JAMES D. JACOBY, D.D.S.

Today, dentists who have taken one or more courses in the subject of hypnosis have been estimated to be in excess of six thousand. There are many dentists who instruct in the practical use of hypnosis for dentists. While basic, introductory courses in the general subject of hypnosis can be taught by anyone, it takes a dentist to teach the practical use of the technique to a dentist.

A group will learn something from every speaker who may present himself, his specialty and his technique. One of the principal obstacles in the routine use of hypnosis by the profession has been the lack of confidence by the doctor in himself. (Heron) Also the doctors claim, "lack of time to train the patient." (Heron) Four years ago, (Jacoby) it was pointed out that, in my general practice of dentistry, all patients are not consciously directed to partake in hypnodontics. This is still the policy today.

A careful tabulation was made of 308 patient-subjects. For these patients, a definite, acknowledged effort was made to obtain a recognizable trance state by a definite expenditure of time and effort. *Specific techniques* were utilized in *generally accepted* hypnodontic therapy.

Statistics were compiled which include age and sex of patient, method of first induction (personal attention or tape-recorder), type of dental service performed, estimated depth of trance, and other factors.

Number of Hypnodontic subjects included in this study	308
Number of hypnodontic sessions (or about four sessions per patient)	1214
Number of boys (under seventeen years of age)	24
Number of girls (under seventeen years of age)	36
Number of adult males	91
Number of adult females	157
Number of patients referred only because of hypnodontics	76

Types of dental services:

Surgical (exodontics)	175 appointments
Operative	749 appointments
Prosthetic	58 appointments
Periodontic surgery	60 appointments
Endodontics	14 appointments
For suggestive conditioning only	158 appointments
Total	1214 appointments

*Presented before the Southern Californial Hypnodontic Society, Los Angeles.

The last item "for suggestive conditioning only," includes patients (1) "referred for hypnotics" or (2) whose attitude at the initial dental appointment was such that it was deemed advisable to train or to "condition" the patient to accept dental service prior to any dental treatment or (3) it was suggested to the patient after several dental appointments that, if he listened to the "relaxation recording," he would be more comfortable during succeeding dental appointments.

Technique used for first induction:

1. Personal attention (3 to 45 minutes each; generally 45 minutes).. 145 cases
2. Tape-recording (plus a minute of personal instruction by dental assistant or by self).....163 cases

However, like most statistics, these figures demanded further study. The comparison was startling to one who felt that most of his patients had been successfully trained in private by the recorder while he was performing routine dental services.

Study of the "date column" of the records indicated a normal reaction to suggestion on my own part: following a lecture, a course either given or taken or the observation of the technique of another operator, dentist, physician or psychologist, several weeks of "personal attention" in patient conditioning resulted. That which was learned or observed, was tried. Then the appointment schedule and personal preference must have prevailed and dozens of cases were "exposed" to the tape-recorder with extremely few exceptions. (Moss)

Where the tape-recorder could be used to save time, energy and repetitious work, it was used by preference.

In cases of emergencies, hysterical or extremely nervous patients (Ament) or for children, a session of personal instruction and attention is the necessary method.

It is also of interest to note that the number of *patients* studied is approximately one-fourth the number of directed hypnodontic *sessions* —or four *directed sessions per patient.* After the fourth appointment, including the training session, a definite, directed suggestion for the patient to relax evidently did not seem to be required. This conclusion agrees precisely with the experience of other hypnodontists who have remarked on this point.

It is possible that many of these subjects may have been in an *operable trance* during succeeding appointments. If so, considering an average of twenty appointments to complete a case, if we multiply the number of hypnotic appointments (four) in this study by a factor of five, a total of six thousand dental appointments were performed under hypnosis.

The importance of depth of first trance may be emphasized by some operators and instructors.

Many dentists have reported that depth of trance in the first appointment or in any appointment is a very unimportant factor. We concur.

The average depth of trance as estimated by the operator is always a questionable factor unless an attempt is made to carry the session out to the stage of proven hypnoanesthesia and amnesia. Regardless of method, it was estimated as follows:

1. Refused all instruction following introduction of the subject	4 patients
2. Refractory: in my opinion, patient did not enter trance	6 patients
3. Hypnoidal stage	53 patients
4. Light trance	63 patients
5. Medium trance	121 patients
6. Somnambule	61 patients
Total	308 patients

Adding the first two together, ten out of 308 gives less than 3% failure to achieve some hypnotic control.

Examples of the reasons why a particular technique was used for first induction are easy to select:

Case #1. Woman, Mrs. R.D., age 42; referred by her general physician upon the recommendation of her psychiatrist for dental hypnotherapy. Physician, by telephone, related history of extreme dentophobia, completely neglected dentition, two miscarriages, periodic asthmatic attacks diagnosed as psychogenic, recently divorced. All hope that "hypnodontotherapy" will take care of the dentophobia and permit dental rehabilitation, will decrease or eliminate the asthmatic attacks and decrease her obesity. Tightness of her neck muscles were a constant annoyance.

The patient was given an appointment which would allow the dentist fifteen minutes of discussion, with promises of cooperation and understanding; he should also infer that results would be based entirely upon the ability of the patient to accept suggestions. There must be a quotation of the fee schedule based upon the time to be expended in the patient's behalf. This case was a departure from the usual office policy which required complete examination, consultation and contracture of the case prior to hypnotherapy.

Following the introductory discussion, the dental assistant made the patient comfortable in an operatory, turned off the overhead light, lighted the clamp-on light (7½W) ahead and above the patient, turned on the intercom switch in order to monitor the room at her desk, closed the door and started the tape-recorder placed in a cabinet near her desk. (The sound of the recorder can be directed to the speaker in each room including the recovery room at the same time that music from the radio

or record-player can enter all other rooms.) Following this session, the patient was dismissed.

At the next appointment, the patient appeared, willing to continue. Seated in the chair, she was asked to cooperate by looking at the tiny light ahead of her; to take a deep breath, let it out, close the eyes, tell herself to "relax deeply." Then to keep the eyes closed until she was asked to open them, that with each breath she would relax more and more; instructions were given for future dental sessions with "relaxation" and that she would look forward to each dental appointment with pleasant anticipation rather than with apprehension.

After each appointment, she would leave the office wide awake and alert, rested and refreshed.

At this appointment, we took full x-rays, study models and charted the mouth by oral examination.

The patient was also told that a good feeling of well-being would be noticed after each appointment; that she would be optimistic; that the beneficial suggestions of her physician and psychiatrist would be easy for her to follow. Note, please, that suggestions regarding dentistry are definitely permissable. For other physical and mental-health effects, we defer completely to those trained in such fields. This is not only proper but eliminates possible criticism from those who might be very ready and exceedingly anxious to criticize.

At the second session, hypnoanesthesia was obtained on the right thumb after a half hour of suggestions. The anesthetic effect was then transferred to an upper molar. Anesthesia lingered in the thumb for one hour following the dental appointment, as taught by Burgess. Breathing during the session was deep, rythmic and painless, a phenomenon that pleased the patient.

Dental services are progressing most satisfactorily. The patient looks forward to her dental appointments. She has lost eleven pounds and has experienced neither an asthmatic attack nor a migraine headache since. Two months have elapsed.

Case #2. Mr. L.B., age 27, It was Saturday noon. The last patient to leave, a lady, returned with a young man in tow. He had "enjoyed" a toothache for two weeks of more. His last trip was to the Islands. While he dreamed of being on Waikiki Beach, an x-ray of an upper third molar was taken, the tooth was removed (2 c.c. of local anesthetic was used); the patient was dismissed with several *good* post-hypnotic suggestions. Time elapsed: 17 minutes.

Case #3. E.L., boy age seven, with extreme reticence to sit in a dental chair. He was "relaxed" in the reception room and walked to the operatory where several carious teeth were partially cleaned out and sedative-

filled; two deciduous teeth were extracted. The boy was then returned to the reception room and told to accept the fact that dental service had been performed . . . and more would be done in the future in the same way. Now he refuses to enter the office unless he is promised that he can go to sleep and dream about Bugs Bunny while his teeth are being cared for.

The only test used for suggestibility or "hypnotizability" is to ask the subject to look at an object, a light or a spot, for five seconds. Most people can do this. These subjects are all hypnotizable. Anyone who cannot look at a "spot" for five seconds without looking away is a poor hypnotic subject.

Summary: Interest of the dental profession in the use of hypnosis for dentistry has increased a thousandfold in the past several years. Periodicals and dental meetings reflect this increased interest and acceptance.

Discussion is made of 308 dental patients in 1214 appointments, including all types of dentistry. The use of the tape-recorder in patient-conditioning is recommended to conserve both time and energy of the dentist.

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HYPNOSIS IN ASTHMA

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Although asthma is considered as an allergic manifestation, there is still no definition of it which is universally accepted. From the strictly orthodox point of view it can be described as a clinical syndrome marked by paroxysmal dyspnea and characterised by "wheezy" breathing. The basic functional disturbances are spastic contracture of the bronchial musculature and an over production of viscid mucus. From a psychiatric point of view, asthma may be described as a nervous breakdown of the respiratory apparatus. All the essential elements are present; the weeping mucous membrane, the tensed bronchial tubes and the disturbance of sleep. It represents a non-physiological response of the respiratory system to the human sorrows, hates and fears that beset mankind. The average asthmatic patient is unable to express such basic drives as anger and aggressiveness and suffers from an inner turmoil covered by a facade of pleasantness. Like the chorus girl who has to bear up and smile, even though she has just heard her mother is dangerously ill. Asthma then, in my experience, results from the effects of inner stress which have remained unresolved by either outward action or successful adaptation.

Autonomic Hyperactivity

In asthma, there is an instability and excitability of the autonomic nervous system—an exaggerated response to tension and anxiety.

Physiologically, asthma would seem to be the result of parasympathetic hyperactivity which, as already stated, produces spasms of the bronchial musculature and an over-secretion of viscid mucus from the submucosal glands. Afferent fibers pass into the vagi from them and it is these glands that are involved in mental states associated with the bronchial secretions which occlude the bronchi.

In all cases where autonomic visceral reflexes are involved, conditioned reflexes are in evidence. In many of them the psychic element either determines the incidence of the attacks or explains the entire condition. The conditioned reflex, once established, continues to operate after the real exciting factor has ceased to apply. Conversely, if the patient can be kept free for several months, he is quite likely to remain free for the rest of his life. Hypnosis acts by decreasing the hyperactivity

of the vegetative nervous system and by interrupting the conditioned reflex arc which can precipitate an attack.

Structural and Visceral Changes

Repeated attacks of asthma can quickly give rise to skeletal changes in the chest cage and spine arising chiefly in the first growing period—pigeon breast, kyphosis, and visceral effects—emphysema, bronchiectasis, right heart strain—inevitably leading to restriction of occupational opportunity and even to diminished expectation of life.

Predisposing Factors

The constitutional factor is important and on the basis of clinical impressions the asthmatic patient displays a distinctive personality pattern. Linford Rees, in a study of 441 asthmatics, found that in comparison with a control group they were more timid, sensitive, anxious, obsessional and unstable. The asthmatic group found greater difficulty in adapting themselves to environmental changes.

The factor of inheritance has always to be appraised, as in approximately 50 per cent of cases there is a family history of asthma, eczema, and seasonal or perinnial rhinorrhoea.

Another factor is that of identification. The patient may take over from another person, especially someone to whom he is much attached, the asthma symptoms.

Precipitating Factors

Menstruation and Sexuality

Some women have attacks of asthma before a menstrual period. The spasms are induced by the fear that they may be pregnant. One woman stated, "If I have a child and I am ill, who will look after it." Another one stated, "I am afraid of having a child in case it might develop asthma."

Women who long to have a baby will get attacks when a period starts because of the disappointment of not becoming pregnant.

One woman wanted a child, but not until she could furnish her rooms. She and her husband slept in separate beds. She stated, "If my husband comes near me, my chest starts to tighten up. I had a nightmare last night in which I was being chased." The meaning here is very clear.

Children can get excited through masturbation which in turn brings on attacks.

Clinical Types of Asthma

In reviewing 18 cases of fatal bronchial asthma Robertson and Sinclair (1954) classified them according to etiology into three groups: psychological, allergic and infective. A psychological background was present as either a primary or secondary factor in 12 cases and one case only was due to allergy alone.

Allergy

Some allergists hold that the allergic reaction is the only stimulus responsible for asthma, while others say that allergic disposition may be present in a patient but under normal conditions the allergy may be latent or mild and yield to conventional treatment. Under the stress of emotions, however, it can become chronic and disabling. When this occurs, it can be eliminated by therapy of the emotional factors even though the skin-sensitivity tests remain the same. Bignall (1953) says "Skin testing seldom aids in diagnosis and treatment. It is generally more profitable to explore the patient's mind and his environment than the lungs, sinuses or hypersensitivity."

One physician quotes a case of a patient who is sensitive to ragweed pollen, but who, interestingly enough does not have these attacks when his mother-in-law is out of town. The mother of one of my asthmatic patients told me that her young son while visiting his cousin engaged in a pillow fight. The pillow burst open and feathers flew all around the room, but the boy did not get asthma. Yet at home because he was allergic to feathers; he slept on a kapok pillow.

A doctor's wife was sensitive to cats dating from the time she was 12, when her own cat had to be destroyed. After a few sessions of hypnosis the so-called asthma-produced allergy disappeared. She told me that she, her husband and two children were dining out one evening and a cat came and rubbed itself against her leg. She said she couldn't have cared less but her husband almost had kittens.

Infective Type

A high proportion of asthmatics state that head colds and bronchitis are regularly associated with attacks of asthma. In England and Wales during the years 1938–49, deaths from asthma accounted for roughly 3,000 deaths per annum; approximately 96% of those dying from this cause were over 30 years of age, when infection of the respiratory tract is common. When a patient states in his history that bronchitis is certain to cause asthma, under hypnosis I say to him, "Look at all the people who get bronchitis, they don't get asthma, and there is no reason why

you should. I can't stop you from getting bronchitis, but you will never get tight and wheezy with it." Another thing I suggest, "whatever phlegm you have got is going to dry up and you are not going to make any more." Later on when the patient does get an attack of bronchitis and does not get asthma with it in spite of being apprehensive that he would, he loses his fear of it.

You will recall that I stated that the deep submucosal glands produce a light frothy or thick tenacious sputum. You will recall also that these glands are under autonomic control of the vagus, which can be influenced under hypnosis. Patients who live under constant tension for years and then get bronchitis and develop asthma, will go on making phlegm. Phlegm leads to coughing and coughing leads to oldema and emphysema, making the patient puffy and breathless on exertion.

A nurse aged 53 with asthma of 22 years duration was puffy and breathless on exertion and was bringing up 3–4 ounces of phlegm each day. Three months later, there was scarcely any phlegm and she could walk quickly without getting puffy and breathless.

Another nurse aged 36 with asthma of 2 years duration, which started with bronchitis, had a patch of bronchiectasis. After some months she said, "I produce no phlegm and yet I used to produce half a tumbler a day."

Psychological Point of View

Since we take care of people and not just their diseases, we cannot but help deal with their emotions. The more knowledge the doctor possesses of the emotional factors in his patient's illness, the more capable he will be in dealing with him and his disease. In the asthmatic, alterations of the psyche is the rule rather than the exception; worry, excitement, psychic trauma and conflict can easily precipitate the first attack.

One woman started her asthma shortly after she was married. She and her husband lived with his mother who was a bad heart case. She missed her first period within two weeks of the wedding and when she told her mother-in-law, this benevolent creature replied, "They will be saying in the village, that's the way you were when you married my John."

Hypnotherapy

Children

Children, as you know, are very open to suggestion for hypnosis and with the very young ones, I do magic. First I get the parent to simulate catalepsy of the eyes, then I ask the child to try it. If the child is

successful, I then turn rapport over to the parent who induces light hypnosis at bed time. If the child is too young or unco-operative, then the parent talks to the child one hour after it is asleep.

Having ascertained from the patient and from the parent the list of situations that provoke attacks (excitement, respiratory infections, worry about school exams, etc.), suggestion is made to the child that these situations will no longer make it tight and wheezy.

Adults

In the investigation of an adult asthmatic, one can hardly do better than obtain the answer to two of Dr. Halliday's questions.

1. Why did the patient become ill when he did?
2. What sort of man is he, who has this illness?

Actually, it is one of the curious things about human nature that intelligent men and women so seldom see the causative connection between worry, conflict, unhappiness or other emotional torment and their illness.

Case: Male, age 49, electrical engineer with asthma since May 1952. He has slept in a chair for the past year. One Christmas he spent nine weeks in the hospital. Another Christmas six weeks after his mother died, he had to leave the house he had been living in and the authorities found him a flat above a butcher's shop. The entrance to the flat was through a back yard which was filled with old boxes and tins, and very muddy after a rain, and it rains a good deal in England. He said, "I feel ashamed when I have to invite my freinds in, especially at Christmas time, they tell me it is like going slumming. I went to the hospital hoping and praying they would find something wrong with me. The tests were all negative, but I had injections for five weeks and I had a bad time." I had some difficulty in convincing him that his sensitivity over the back entrance to his flat was the cause of his asthma. He was advised to clean up the back garden, put down a path of crazy pavement, plant flowers and in general make the place presentable. While he was attending to this he had seven sessions from February to May. He lost his asthma and on his last visit he stated "the back yard looks lovely and the butcher says he's never seen anything like it."

Psychic Trauma

Psychic trauma, as already stated, is common in the psychogenesis of asthma and it often comes on after an operation particularly if the patient is afraid of having it.

Case: Female, aged 34, a Malayan barrister had asthma for two years following an operation for fibroids. "I was going to have an operation for fibroids on the first of June. I was terrified at the thought of it.

I made my will as we always do at home, because in Malaya an operation is the last stage of an illness. Other patients from my ward went out in the morning for their operations, and each one said good-bye to me. This made me more terrified. I had an injection which made me very dry but it did not remove the terror. Two weeks after leaving the hospital I developed my asthma. I was still tensed-up from the terror of the operation." Within four months she was completely free from asthma. This was in 1951 and there has been no recurrence.

Another young woman, a ballet dancer, developed asthma after an operation for appendicitis. I asked her why she was afraid of the operation and she said, "My brother died after the same operation."

Mental Conflict

Mental conflict is not only the basis for neurosis but it also fits in the etiology of asthma. Two heads may be better than one but two minds are worse than one.

Case: Female, aged 28, a dietition had asthma at 7, fairly free from it after that but of severe duration for 2½ years.

"Two and a half years ago I fell in love with a chap and we wanted to get married. I am a Roman Catholic and he is a Protestant and he would not change his religion. We broke off seeing each other but I think of him when I go to bed at night."

I told her it was no use starting therapy until she decided which of the two she wanted, Eric or her religion. As she decided for her religion, I had the task of making her indifferent to Eric but at the same time I filled her subconscious mind with thoughts of nice Roman Catholic boys whom she knew. Thanks to the power locked up in the subconscious mind, she got rid of Eric, her asthma, and she had already begun to think of her new Lochinvar.

Conclusion

Asthma is a symptom in which personality disturbances are due to psychogenic factors, therefore there must be a return to the treatment of the sick patient instead of the symptoms from which he suffers. If we are to carry out treatment efficiently, we cannot afford to neglect any aspect of the patient's personality. The patient has a mind and the patient has a body, but we must also think of him as a social being in relation to his personal and material environment.

Prognosis

At Kings College Hospital, asthmatics are divided into five groups ranging from mild cases to cases of status and extreme emphysema.

These factors must naturally be taken into consideration. But from the hypnotherapists point of view—is he susceptible to hypnosis? What sort of personality is he? The prognosis is always better when one deals with a good personality rather than with an obsessional or anxiety prone patient. What were the underlying factors that started the condition and what is the life situation that keeps the condition going. The course of recovery is seldom at a straight line but much more often it is of wave form, the direction of the wave being towards normality.

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BOOK REVIEWS

Marmer, Milton J. *Hypnosis in Anesthesiology*, Charles C Thomas, 1959, 150 pages

Dr. Milton Marmer's book, "Hypnosis in Anesthesiology" is a specific and necessary contribution to the hypnotic literature. As hypnosis continues to become a more useful modality in the therapeutic approach to patients, there is an ever increasing need for a consolidation and authoritative presentation of material which can serve the practitioner in his chosen specialty.

Dr. Marmer's book delineates extremely well the role that hypnotherapeutic technics have in anesthesiologic patient care. He identifies an apparent hiatus in the anesthesiologist-patient relationship as being one that is particularly amenable to the hypnotic approach. There is no doubt that a major fear of many patients anticipating surgery is based on the lack of understanding. This results in an inability to transfer concern for their welfare to the responsible hands of the obligated physicians. The hypno-therapeutic technic is of great importance in accomplishing this transfer.

Dr. Marmer is clear and concise in indicating wherein he has used hypnosis in his specialty. This type of experience is extremely important in guiding anesthesiologists in their approach to similar problems.

The last chapter of the book is particularly applicable. It offers many guides and suggestions which can serve as springboards for the relatively uninitiated hypno-therapist to improve his technic and understanding.

The format, design and arrangement of the book contributes to its attractiveness.

"Hypnosis in Anesthesiology" is a needed and welcome addition to the armamentarium of the anesthesiologist who wishes to utilize hypno-therapy in his practice.

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Schneck, Jerome M. Editor, *Hypnosis in Modern Medicine*. Springfield, Ill. Charles C Thomas. 1959, Second Edition, 389 pp. $8.75.

This book is a well organized collection of informative articles dealing with the use of hypnosis in medicine and dentistry. The Editor, Dr. Jerome M. Schneck who is the founder and Past President of the Society for Clinical and Experimental Hypnosis, has done a very competent job in selecting and assimilating material written by outstanding authorities.

The book opens with an interesting article on the "History of Medical Hypnosis" written by Dr. George Rosen, the editor of the Journal of the History of Medicine and Allied Sciences. This is followed by a chapter by Dr. Bernard Raginsky of the Montreal Jewish Hospital on "Hypnosis in Internal Medicine." He describes many and varied medical conditions which have been treated with hypnosis and concludes that "some use of hypnosis may be made in any medical condition which has an emotional component involved. This holds true," Raginsky says, "whether the emotional disturbance is a primary factor in the production of the illness or is merely the result of organic distress."

The section on "Hypnosis in Surgery" is by Dr. Harold Rosen of Johns Hopkins University and Hospital. In it he discusses the use of hypnosis with surgical patients for the production of anesthesia, the differential diagnosis of psychiatric from surgical conditions and the investigation and treatment of emotional factors which seem to create surgical problems pre-and-post operatively. Dr. Anthony Owen-Flood, Consultant Anesthesiologist, Hornsey General Hospital, London, in the next chapter discusses in detail the use of hypnosis in anesthesiology. He describes: (1) its use as a preoperative sedative and as an induction to general anesthesia, particularly with children. (2) the uses and limitations as an anesthetic. (3) its use in post-operative therapy for such problems as breathing, vomiting, passing of gas and urinary retention.

The use of hypnosis in dermatology has long been a neglected field. Dr. Mehl McDowell of the Los Angeles Institute for Psychoanalysis in writing on this subject avoids referring to specific skin disorders. "I believe," McDowell says, "we can usually consider the skin pathology as a local complication or manifestation of a decompensation of the total organism's adaptive economy." He feels that the total patient rather than the particular skin disorder should be treated and if treatment is successful the skin pathology will disappear. He states that this can be accomplished without hypnosis but adds that with certain patients "the use of hypnosis may make the difference between therapeutic success or failure." This is a significant point.

"Hypnosis in Obstetrics and Gynecology" is written by Dr. William S. Kroger of the Chicago Medical School. He discusses the use of hypnosis as an anesthetic agent in obstetrics detailing its advantages and disadvantages, its indications and the management of the pre-natal period, labor and delivery. He discusses too, the therapeutic application of hypnosis in functional gynecological conditions.

The section on psychiatry is dealt with by the Editor, Dr. Schneck who is Clinical Associate Professor of Psychiatry, College of Medicine, State University of New York. In this chapter Dr. Schneck presents the approach to the patient the place hypnosis has in psychotherapy and its role in the study of personality. In addition, he deals with such topics as induction, transference and hypnotherapeutic and hypnoanalytic techniques.

The chapter on "Hypnosis in Child Psychiatry" is written by Dr. Gordon Ambrose of the Child Guidance Clinic, Prince of Wales Hospital, London. In discussing the therapy of children for various types of ailments Dr. Ambrose makes this most important point, "There is no doubt that hypnotic suggestion has achieved its most spectacular results in the anxiety-tension of children. It would appear that hypnosis cuts across this anxiety-tension reaction and DETENSIONIZERS the child, allowing ordinary methods of therapy to be rapidly assimilated. In selected cases, no other modern therapy can achieve such rapid and hopeful results." Many therapists would be in agreement with this finding which is applicable to the therapy of adults as well as children.

The section on "Hypnosis in Dentistry" is presented by Dr. Jules Weinstein of Harlem and Triboro Hospitals, New York. He writes "knowing why hypnosis works, when it should be used, how to make it most effective and when to discard it after it has served its purpose, has been the intention of the author." In dealing with dental problems, Dr. Weinstein has accomplished this goal admirably.

Dr. Bernard E. Gorton of the Evangelical Deaconess Hospital, Detroit gives an exhaustive report on the "Physiological Aspects of Hypnosis", delineating what hypnosis can and can not do in altering normal and abnormal physiology.

The final chapter, "Instruction in Hypnosis", by Dr. William T. Heron, Professor of Psychology, University of Minnesota deals with some of the problems encountered in teaching hypnosis to professionals.

The Editor has written a "Supplementary Review of Literature." This covers new literature which is considered especially important or essentially representive of work in a given field.

This comprehensive and informative book is written in a clear and readable form. Throughout are many illustrative case histories with the psychodynamics together with the hypnotic techniques and therapy employed. There are numerous, useful references. The Editor and the eleven authors are to be congratulated for this fine contribution.

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Reiter, Paul J. M.D., *Antisocial or Criminal Acts and Hypnosis: A Case Study.* Charles C Thomas, Springfield, Illinois, 1958, Pg. 219

Although Dr. Reiter focuses on a single criminal case, his monograph deals with the general question of whether antisocial behavior may be induced by means of hypnosis. A brief but thorough review is given of experimental evidence, prevailing opinions, and case histories relevant to this issue. The major portion of this book is devoted to an extensive account of a criminal case involving hypnosis. The defendent, H, had been hypnotized and was the perpetrator of the crimes. Dr. Reiter, who was called in to examine the defendent, had minimal contact with the hypnotist. The information contained in this report is based almost exclusively on H's account given in hypnosis and evidence obtained by the police.

The defendent was a young man who was sent to prison for collaboration with the Germans during their occupation of Denmark. At this time in his life he found himself to be extremely disillusioned and depressed. In prison he encountered a man whose professed knowledge of the occult fascinated him. The two became friends, worked in the same shop, and shared a cell. From that time on, and for many years thereafter, they "experimented" with yoga and hypnosis (though they never used this term). The hypnotist ("N") succeeded in placing the defendent (H) in a deep somnambulistic trance and convinced him that the means to salvation was through a renunciation of material objects and a concentration on matters of the spirit. N, the hypnotist, also allowed himself to be placed in trance and during one of these sessions, H, the subject, was told his "guardian spirit", X, was speaking through "N". The guardian spirit informed H that this life was his last possible reincarnation, and that if he did not abide completely by his commands he would be eternally damned. H seems to have completely accepted these pronouncements, and carried out all demands to the best of his ability.

Eventually, H came to accept "N" as the voice of X regardless of whether he was in trance. Furthermore, H occasionally heard and saw his guardian spirit X when "N" was entirely absent. During their stay in prison, at "N" 's request, among other things, H cut off all relationships with others, gave "N" his watch, and became a vegetarian (thus allowing "N" to have twice the meat rations to which he was entitled). The guardian spirit informed H that his mission in this life was to achieve the unification of Scandinavia. In their hyp-

notic sessions—both in and out of prison—"N" caused H to "practice" in fantasy antisocial acts, among them matricide. The rationale for this was that H was not subject to the same moral code that bound other mortals, and thus must be able and willing to commit any act that would further their higher end.

After their release from prison the men continued their relationship on the same basis—still performing "experiments" in yoga and hypnosis. "N" introduced H to a girl and instructed him to marry her, which he did. Prior to the wedding, "N" ordered H to allow him ("N") to have sexual relations with the girl. H agreed and when the girl resisted H told her that he would not marry her if she did not permit it. She acquiesced and the marriage took place shortly after. "N" made many demands upon the married couple such as the requirement that H turn over most of his salary to him. H almost invariably fulfilled "N" 's demands. The experiments continued, as did the discussions of how to unify Scandinavia. Money was needed to form an armed group in order to carry out their political aims. To get the money, a bank robbery was carefully planned. H executed the crime and turned the money over to "N". A second robbery was planned which H also perpetrated. During the robbery H shot and killed two people and was apprehended soon after.

In his account of this case, Dr. Reiter places much emphasis on the hypnotic relationship which existed between the two. He feels that by means of hypnosis "N" systematically changed H's character and induced an entirely new "secondary" personality. He concludes: "It is possible from this case to deduce the sad moral that a sufficiently skillful and cunning hypnotizer, given suitably favorable circumstances, will still have the possibility of committing serious crimes with the help of another person whom he has by means of hypnotic training formed into a useful tool for carrying out the crimes without himself running the risk of discovery and subsequent punishment." (p. 215)

In our opinion Dr. Reiter's formulation of the case is oversimplified. The concept of an artificial induction of a complete secondary personality is, from the psychiatric point of view, dubious. Dr. Reiter's report of the events in the case is based almost exclusively on H's own account which was given under hypnosis. Dr. Reiter claims that H's statements are veridical because they agree with the evidence, and because he gave H suggestions that he would be unable to lie. We agree with the former criterion but take issue with the latter. Hypnotic suggestions are not inviolable as several studies have shown.

Another example of Dr. Reiter's oversimplified approach is his faith in the phenomena of the locking suggestion, i.e., suggestions to prevent subsequent hypnosis. In this case, "N" repeatedly suggested to H that he would be unable to enter trance with anyone but "N". Aside from other evidence—including our own work—which indicates that locking suggestions are assailable, the very fact that Dr. Reiter succeeded in hypnotizing H demonstrates that "N" 's locking suggestions were not effective. Another criticism of Dr. Reiter's account is that the role of "N" in the entire case is discussed solely from the point of view of H. "N" is presented as an unscrupulous but clever criminal who masterfully manipulated H's personality for his own personal gain. And yet Dr. Reiter himself reports that "N" was of low average intelligence, whereas H was of high intelligence. Furthermore, although H's personal history and psychodynamics are extensively reported, the personality of "N" is barely considered. In our opinion the events in this case can be best understood in the light of the re-

lationship between the two men. A one-sided account such as Dr. Reiter's is hardly an adequate presentation of the case. In view of Dr. Reiter's one-sided method of investigation, and his apparently naive conception of hypnosis we cannot concur with his conclusion that "It is true that there were 'various influences used, among them hypnosis': but without hypnosis all the other influences would have been in vain." (p. 210)

Two important issues raised by the monograph are: 1) whether antisocial behavior can be hypnotically induced, and 2) if it can, with whom does the responsibility for these acts lie. Dr. Reiter marshals considerable evidence to support the argument that the hypnotic subject may be the hapless tool of an unscrupulous hypnotist who is then solely responsible for whatever crimes are committed. We will briefly outline here our own views which will be more extensively discussed elsewhere.

The data generally used to support the position which Dr. Reiter takes falls into two categories: actual criminal cases, and laboratory experiments. With regard to the latter it appears to have been demonstrated in the laboratory that hypnotic subjects will perform antisocial and self-injurious acts at the suggestion of a hypnotist. However, this behavior must be considered in the context of the total situation. Volunteer subjects are often motivated to tolerate dangerous and stressful situations for the sake of advancing scientific knowledge, as for example, in some medical research. Moreover, the laboratory hypnotist is generally known by the subject to be a reputable investigator who will undoubtedly insure the safety of all involved despite the appearance of the situation. In our opinion, no situation which is perceived by the subject to be a scientific experiment can validly test the question of the possibility of hypnotically inducing antisocial behavior. A more adequate test would involve a situation which is more "real" to the subject, and where the hypnotist seems to have some clear-cut personal profit motive for suggesting the antisocial behavior.

In the past 25 years there have been only three documented cases reported of actual criminal behavior involving hypnotic suggestion. If Dr. Reiter's thesis is correct that a "cunning hypnotizer" can form another individual into a "useful tool" for perpetrating crimes, then it is surprising that there are so few cases of this nature reported; especially since the lay public has been extensively exposed to views such as Dr. Reiter's. All three of the reported cases are marked by an intense interpersonal relationship between subject and hypnotist which preceded the hypnotic work. As in any intense relationship the motivation of the individuals involved are extremely complex, and the behavior of each with regard to the other must be understood in the context of the total relationship. An explanation which purports to account for such behavior by singling out one aspect of the relationship—i.e. hypnosis—must be viewed with skepticism.

With regard to criminal behavior, it is not extraordinary for one individual to perform antisocial behavior which benefits another. The only unusual feature of the Reiter case is that hypnosis played some role. We would not ascribe the subject's motivation to hypnotic influence, as Dr. Reiter seems to do, but would relate it to the kind of relationship which existed between H and "N". In this case the relationship seems to have been characterized by a shared psychotic system, and there are some indications of latent homosexuality. The role that hypnosis plays is to disguise for the subject his own motivation and to create a situation which allows the subject to, at some level, disclaim responsibility for

his actions. We will refrain from committing ourselves on the question of legal or moral responsibility which, in any case, is not for a psychiatrist to settle. In our view, antisocial behavior such as H's could have been performed without the introduction of hypnosis.

Despite our methodological and theoretical criticisms, Dr. Reiter's book is probably the best single study available of a criminal case involving hypnosis. The case is a fascinating one, and at times borders on the incredible. The book is well worth reading; but the reader should bear in mind that events depicted may have alternative explanations.

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Chertok, L., *Psychosomatic Methods in Painless Childbirth: History, Theory, and Practice*. Translated from the second French Edition. Pergamon Press, New York, 1959. 266 pp. 587 references. Author and Subject indices.

This is by far one of the most important books directed toward the use of suggestive or non-pharmacological methods of producing analgesia and anesthesia for childbirth. The author, who has an extensive background in the research and clinical aspects of hypnosis, reviews the present day hypnotherapeutic approaches as well as those employed during the last century. He compares Read's method of Natural Childbirth with psychoprophylactic Relaxation, Schultz's autogenic training, progressive relaxation, and various combinations of these psychophysiological methods.

Particularly instructive is the clarification of the psychophysiological methods used for alleviation of the pain of childbirth in the Soviet Union. This segment of the Russion Scientific literature has received scant attention in this country.

The excellent chapter on the role of the husband as an integral factor during pregnancy, especially during labor and delivery is stressed. This important area has also been neglected in American Obstetrics.

Educational versus non-educational, hypnotic and non-hypnotic technics are compared, and there is a comprehensive review of the world literature on these methodologies. These afford the reader an excellent opportunity to familiarize himself with some of the recent trends in psychosomatic obstetrical analgesia and anesthesia as used during the last century.

This reviewer agrees with the author that the difference among the various methodologies are more apparent than real, and that identical results are undoubtedly obtained with all techniques. He consistently helps the reader to maintain the distinction between the empirical values of a given method and its theoretical rationale.

The first part of the book covers the early history of Mesmerism (hypnoanesthesia) both in surgery and obstetrics. He briefly discusses the techniques and results of the hypnosuggestive methods employed in obstetrics at the end of the 19th century as well as those of the last century.

The most interesting chapter for the American reader is a review of the hypnosuggestive methods used for painless labor in Germany and Russia during the early part of this century. Chertok correctly recognizes that the therapeutic results, in most instances, depend upon the intra and interpersonal aspects of the doctor-patient relationship.

There is a critical evaluation of the Read method which is thoroughly discussed. It is concluded that Natural Childbirth has a high content of waking

hypnosis. Schultz's method is also described, but all too briefly. He shows how this technic can be used individually or in combination with Natural Childbirth.

The last half of the book discusses the psychoprophylactic methods as developed by Velvovski. The fundamental principles of Pavlovian physiology are recapitulated. The role of the cortex in childbirth and in perception of pain as well as the nature of childbirth pain are elucidated.

Chertok points out that Velvovski minimizes the role of suggestion as the responsible factor for the success of psychoprophylactic relaxation. However, he notes that other Soviet workers have pointed out the importance of suggestion and/or hypnosis in raising the pain threshold.

Lamaze's application of the psychoprophylactic relaxation method for painless childbirth is also described in detail. Chertok maintains a laudable objectivity in comparing Read and Velvovski's methods, which in reality, closely resemble each other. This reviewer believes that the difference between all of the methods reviewed by Chertok are minimal; each of these are based on highly theoretical psychophysiological formulations. The psychotherapeutic results are achieved by many factors, among which are rapport, cultural attitudes and the raising of the sensory threshold by healthy motivation for motherhood. This reviewer doubts that the exercises and breathing exercises are of value for raising the pain threshold.

Pain is transmitted by two routes, the mental and physical. Chemoanesthesia effectively blocks off the painful afferent impulses to the higher centers. The mental route for pain perception can be effectively controlled in many instances by suggestion and/or hypnosis. Unfortunately, the proponents of various methodologies do not recognize where suggestion ends and hypnosis begins. This undoubtedly accounts for the semantic confusion evidenced by the numerous "labels" for the various techniques; these obscure the fact that the common denominator for all are a favorable mental set produced by belief, confidence and an expectant attitude—all catalyzed by the imagination to induce relaxation. They also fail to consider that even the presence of the obstetrician with the patient in labor has a highly sedative effect.

The only relatively minor criticisms are that the specific techniques for handling women in childbirth are not described in detail; that the discussion on the nature of the pain is inadequate, and that the psychosocial factors associated with childbirth are only briefly discussed. This book is a distinct contribution to the psychosomatic aspects of obstetrical analgesia and anesthesia. It is highly recommended not only to all obstetricians interested in relieving the pain of childbirth, but also for those interested in the research possibilities of hypno-suggestive methods.

William S. Kroger, M.D.

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Arthus Shapiro, M.D., Brooklyn, New York has been appointed a member of the Board of Directors of the American Board of Medical Hypnosis.

M. Erik Wright, Ph.D., M.D., Lawrence, Kansas and Howard W. Marcus, M.D., D.M.D., New York City have been appointed members of the Board of Directors. Dr. Wright will serve as liason contact with the American Board of Examiners in Psychological Hypnosis and a similar position will be held by Dr. Marcus in relation to the American Board of Examiners in Dental Hypnosis.

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